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CIVIL ENGINEERING.

On the Testing of Chain Cables. By FREDERICK ARTHUR PAGET, Esq., C.E.

From the Journal of the Society of Arts, No. 598.

(Continued from page 95.)

THE apparent increase of ultimate strength through successive breakages, thermo-tension, and much of the high static breaking strength acquired through cold rolling, and cold hammering, even through wire-drawing previous to annealing, are referable to an increase of hardness, to an increase of the difficulty of the gliding to and fro, to a resistance to the inter-mobility of the particles, to, in one word, a diminution of elasticity. If the numerous experiments that have now been made on iron do prove any thing, it is that the breaking strength does not indicate the quality—the breaking strength must be taken conjointly with the elongation. The true measure of the mechanical value of wrought iron is simply the sum of the products of the successive loads and the increments of elongation—in other words, the resilience of the bar or the deflection of the beam, or the work performed in producing the stretch or deflection. We thus see the value of Poncelet's symbols T. and T advocated with such ability in England by Mr. Mallet. Upon the just balance of strength of fibre, or high breaking strength, and extensibility or ductility, depends the mechanical or structural value of iron.

The Navy test for chain cables is stated to be the result of a number of careful experiments by the late Sir Samuel Brown, and it was adopt-

ed by the Admiralty in 1831, when chain cables were fairly established in the royal service. The test adopted by the Fench Navy is almost exactly the same, and in Russia and the States it is exactly the same, as both those countries use our own measures and weights. Every chain cable is proved by a gradually applied stress of 630 lbs. for each circular one-eighth of an inch of the area of the bolt of which the cable is made or 11·46 tons to the square inch on each side of the link.

Assuming that a link is subjected in practice to a tensile stress, and as the proof strength is generally fixed at double the working stress, this would correspond to nearly 5¾ tons on the square inch. There is thus a very close correspondence between the working stress assumed for chain cables and the Board of Trade limit of 5 tons to the square inch, imposed about 16 years ago, for both the tension and compression of the wrought iron of railway structures. The chain cable of a ship is also evidently subjected to impulsive forces. It is true that a ship, when struck by a sea, in most cases nearly lifts the weight of her chain, the catenary curve of which thus acts as a kind of water-brake; but a very heavy sea must occasionally bring a sudden pull on the cable, and in shoal water the sudden strain must be almost solely taken up by the resilience of the cable, or rather by the deflection of the series of beams composing the cables. Much security is, however, afforded by the fact that a cable is generally only strained during a brief interval of time. But few cables can stand a sudden nip at the hawse-pipe; and we thus see that lateral as well as longitudinal strength is occasionally required in a cable.

If two one-inch diameter cable bars of average quality, and say, each ten feet long, be put into the hydraulic press generally used for testing cables, the following appearances will probably be observed:—If new, they will take a very slight set under a stress of about 1¼ ton to the square inch, but if this stress be gradually increased, and alternately eased off and put on several times, the set will not increase until the true elastic limit or proof strength of the material be exceeded. In our case this limit will probably be 12 tons to the square inch, which is thus higher by a little more than half-a-ton than the 11·46 tons navy test. At the Admiralty proof stress, each of the bars will have a probably total elongation of more than one-twentieth of an inch, and a permanent set of six thousandths. Beyond this strain the set will very rapidly increase up to, perhaps, two inches, when the bars will break under a load of 24 tons to the square inch. But the phenomenon the most important in its consequences, consists in the contraction of cross sectional area undergone by the bar through the stretch. According to a theoretical investigation by Poisson, the relation of the contractions to the longitudinal elongation should be ¼; and Wertheim's experiments led him to believe that this relation should be ⅓. Cauchy, Stokes, Maxwell, Rankine, and Lamé, have also mathematically investigated this question, and have arrived at results differing from those of Poisson, which were founded on a special atmoic hypothesis. But the permanent sets that show themselves in ductile bodies, like annealed iron, under very slight loads, and the so-called frictions observed by Dr. William Thomson in metals

under tension, would cause this relation of contraction to elongation to differ for every different state of a metal. To Kirchhoff is due a remarkably important investigation carried out in 1859, into the relation of the contraction to elongation under tension of hard steel wires—which may be said to approach the nearest to the ideal of a body possessing equal elasticity in different directions. His experiments, conducted with great delicacy, gave a relation of cross sectional contraction to elongation of 0·294. As we have seen, according to the Admiralty tables, a one-inch cable bolt ought to have an ultimate breaking strength of not less than 21 tons 8 cwts. to the circular inch, or more than 27 tons to the square inch, and the link ought only to break at 34 tons. It is, however, very seldom that these strengths are obtained in practice. The ultimate elongations of the bars or the cables are not stated in the Admiralty tables. General Morin relates that the fine charcoal iron, made at Guerigny by the French government, expressly for chain cables, sometimes elongates even more than one-fifth of its original length before breaking, and this amount is probably the utmost that it is possible to give to wrought iron bars.

When the cable itself is placed under the dead pull of the press, it is tested in three different ways. It is first strained up to 11·46 tons in the square inch sectional area across the double section of the link. While for about three or four minutes under this stress, the cable is subjected at different parts of its length to blows from a round-faced hammer. Different sized hammers are adopted in proportion to the size of the chain, and each fathom generally receives one blow. Each link is then carefully examined. Two or three links are broken up to detect, by its bluish tinge, if the iron has been at all burnt in the working, and also to make some estimate of the quality of the iron from the surface of the fracture, and the other appearances known to engineers. Some differences of opinion also exists, both in France and in England, as to the amount of security afforded by these tests, and whether the test of 11·46 tons on the square inch, and more especially the blows of the sledge, do or do not injure the cable. In 1855 it was attempted to introduce a compulsory government test in France for the chain cables of merchant vessels. A letter was addressed by M. David, an influential chain cable manufacturer at Havre, to the then imperial minister of public works, advocating a compulsory test, from motives of humanity to the ships crews, and of public economy. A system of periodical retesting, for every ten or twelve years that the cable had been at work, was also proposed. The attention of the then minister of the French marine was directed to the statements put forth, and Admiral Hamelin ordered an official investigation of the question. The results shown forth in the report would appear to have proved—at any rate to the satisfaction of the Imperial administration, that—1st. "The proof test of 17 kilogrammes or even of 20 to 21 kilogrammes per square millimetre of section of the link, is not enough either to prove the good workmanship of the cables or the quality of the iron employed; 2d. That a higher proof than 20 to 21 kilogrammes cannot be applied several times to cables without affecting their quality; 3d. That the differences

of useful effect between different presses often lead to error with respect to the absolute value of the tension employed. . . . . The sum total of these results therefore shows, continues the minister, that, on the one hand, an increase of the proof test would not be of much effect in detecting bad materials and workmanship, and on the other that it would be dangerous to increase the test. The required security can only be obtained in a well understood system of manufacture; and, therefore, besides the test in the press, it is necessary to scrupulously choose the special quality of iron required; to accurately examine each individual link after the testing; to break up any questionable link; and to choose the most skilful and trustworthy operatives." In one word, the minister of the marine did not consider a government inspection of chain cables intended for the French merchant service as a practicable thing. It is to be remembered that all the chain cables for the Imperial navy are manufactured by the government.

Now there can be no doubt that the proof of 11·46 tons to the square inch is not enough of itself to test the quality of the workmanship, or, more definitely, the perfection of the welds. For this reason Mr. R. Bowman advocated before the 1860 Committee an increase of the test. It is clear that, as the sides are only tested up to little more than 11·46 tons, and as they would break at only, say, 24 tons to the square inch, less than one-half the sectional area of the iron would stand the test if applied only tensionally. As, however, through the cross-bending strain at the two ends, the link slightly tends to assume the shape of a lozenge, the weld is more severely tested than would at first appear. There is a certain difficulty in detecting a bad weld, upon the nature of which some practical light has been thrown by some experiments of Mr. Kirkaldy's on bars grooved round their circumferences. The matter had been previously investigated by the writers on elasticity, but Mr. Kirkaldy was the first to practically test the question. Bars grooved at any particular part down to a given diameter, gave a much higher ultimate breaking strength than bars of a diameter all through equal to that at the reduced part of the gooved bar. The wider parts on each side resisted the tendency to draw out, and a great apparent strength was thus obtained. The extent of this apparent gain was as much as 37½ per cent. in some of the pieces, while the average gave 18·63 per cent. in favor of the grooved specimen. Here again we see the falsity of taking merely the breaking strength into account, for although the breaking strengths were thus increased, the elongation, and the contraction of area attendant on elongation, were proportionately less. It will thus be seen that a bad weld may be impaired by a strain in excess of the elastic limit due to the quality of the iron and the cross sectional area of the solid metal, and that, although it is thus injured, it may not show signs of the injury. On the other hand, some security is given that a bad weld may be detected, from the fact that rolled iron is well known to be somewhat hardened by being hammered, and the welded-up side of the link would thus be less extensible than the opposite parallel side, and would thereby be rather more strained. It is evident, however, that though the test can scarcely be too high

for the welds alone, the proof of more than fourteen tons to the square inch, proposed by M. David, would clearly be too high for the cable. M. David, indeed, stated that he tested his cable up to this amount, but it appears that the pressure he used was not accurately measured. Indeed, there is no doubt that very few cables would stand the ordinary proof if repeated sufficiently often, or if it were put on and eased off a succession of times, upon the plan shown by Dr. Rankine. As it is, the permanent set taken by cables is, on an average, from 4 to 6 feet in 90. But the best proof that this single application of the test for a short time does not injure good chain cables, is seen in the fact that it has been adopted all over the world for more than thirty years. We are, however in a dilemma. To increase the proof would evidently be to injure the link, while the detection of a bad weld has, in any case, to encounter the difficulties just mentioned. These questions can only be met by a most careful inspection of each individual link. The quality of the iron can also be very closely tested by breaking up two or three links. The most searching tests, however, are the hammer blows given while the chain is under tension. Adapting a well-known and excellent illustration, this will be at once evident when we remember that a 1¾-inch chain cable, made of glass, would give the same ultimate gradually-applied breaking strength as a one-inch iron cable—but it would not be likely to stand the hammer test. On the other hand, a cable of india-rubber, although not to be broken by the hammer, would at last be torn in two by the press. In fact, the hammer test approaches nearer than any other to the kind of work that will have to be done by the cable when at sea. Besides, the mere form of a chain renders it, *per se*, liable to continual shocks and jerks, and this must be encountered by a special quality of material, and that this material has really been used must be shown in the proving house.

Mr. Pope, the surveyor for Lloyd's at Liverpool, gave it as his opinion, before the Committee of 1860, that the navy test was too high, and had a tendency to injure the chain. This might be true for a chain of a bad material, but not for a chain made of iron with the high elastic limit that should alone be used for chain cables. He proposed to test a short piece to destruction, and then to test the entire chain up to half the Admiralty proof. Apart from the expense and destruction of material by this proceeding, there can be little doubt that half the usual test would not detect all the bad welds, and the distinctive peculiarity of a cable consists in the fact that a single bad weld is sufficient to cause the entire loss of the chain.

As we have seen that a cable consists substantially of a series of small curved beams, it would be only a natural inquiry to ask why the sum total of their deflections, represented by the temporary elongation of the cable, and why the total permanent set should not be both registered, and be both taken into account when estimating the quality of the cable. There are, however, several influences that would greatly disturb an accurate deduction. It might, at first sight, be supposed that the defective welds would elongate in the inverse ratio of their areas of solid metal to that of the links. This, as we have seen, is not

the case, and even if it were the case, the action would affect the deductions therefrom by variable and uncertain quantities. The links will also bed against each other to an amount given by the hardness of the iron. There can be no doubt that the extension must be taken into account with the breaking weight, when the quality of a bar has to be estimated. But even with bars, this varies considerably, not merely in different qualities, but also, as was shown by Mr. Kirkaldy, in specimens of the very same brand. These results were also obtained under tensional stresses alone, and when we come to the combination of transverse, tensile and directly compressive stresses to be found acting on a link, the varied ways in which these stresses act on varying qualities of iron would scarcely render the deductions from the elongations and set sufficiently trustworthy. Again, to take an extreme case, if one half of, say, 50 fathoms of cable were made of a very bad kind of iron, and the other half of a very good quality, it would be difficult to draw any right deduction from these appearances. As it is, however, the permanent set is generally registered.

There is probably no metal the strength of which is influenced in such a remarkable way by temperature as iron. As M. Baudrimont showed in 1850, the tenacity of iron is less at 100° C. than at 0° C., but at 200° it is greater than at 0°, and these results have been exactly confirmed by Dr. Fairbairn in some experiments on boiler plates, communicated in a paper to the British Association. At yet higher temperatures this tenacity is of course diminished; and Seguin has shown that iron, the tensile strength of which could be represented by 100 at 10° C., had this tenacity lowered to 90·5 at 370° C., and to 58·7 at 500° C. In the royal dockyards of Woolwich and Portsmouth, the atmospheric temperature during the testing of each anchor or chain is carefully noted, although the proving houses themselves are kept at a mean temperature of 56° Fahr. by means of stoves, which also thus save the water pipes from freezing. This temperature of course falls a little during the winter and rises in summer, as the heat in the shade generally varies in England from about 76° to perhaps 34° Fahr. The action of frost on iron has not been completely investigated; and Dr. Percy recommends that some acurate experiments on the question be undertaken by the Institution of Civil Engineers. The daily observation of practical men has, however, as in so many other cases, preceded the deeper investigations of science. All workmen know that their tools, such as picks and chipping hammers, which have to undergo percussion in frosty weather, are then more liable to get broken. All chains are well known to be more subject to snap under the same circumstances. There is always a notorious increase of accidents through breakages, both in the permanent way and rolling stock, of railways during frosty weather. It is stated that during the severe winter of 1860–61, 498 rails were broken on the Chemin de Fer de l'Est, from the 11th December to the 31st January inclusive. No less than 258 were broken from the 21st to the 25th of January, during which period the thermometer descended to —7·8° and even to —16° centigrade. General Morin relates that during the northern campaigns

of the first empire the artillery veterans used to believe that wrought iron was subject to freezing, and after the long winter bivouacs they never began their day's march without striking the gun-carriage axles in the direction of their length, and the vibration thus produced was said to "thaw" the iron. An intense cold is also said to have enabled the French garrison of Hamburg to disable the cast iron siege guns, by knocking off the trunnions before evacuating the place. Mr. Lenox stated, in evidence before the 1860 Committee, his belief that a cable would stand a test in warm weather that it might not in cold. The crews of the fishing vessels on the coast of Nova Scotia find that the cold renders their cables so brittle that a length of hempen cable is used for the portion out of the water, while the anchor end is kept from the vicissitudes of the atmosphere by the usual average temperature of the sea. A few experiments made by Mr. Kirkaldy showed that the breaking strength of a bar is slightly reduced by freezing when a gradual breaking load was applied, but that this difference between a frozen and an unfrozen bolt is much more increased by a suddenly applied load, being 3 per cent. less when frozen. The usual way adopted by French engineers to test rails is, as we have seen, to prove a percentage of the lot by means of a falling weight. Some tests were carried out a few years ago by M. Couche, on a number of rails, of very good quality, from the Anzin works. The monkey weighed 300 kilogrammes, and the distance between the supports was 1m. 10. When the thermometer varied from —4° C. down to —6° C. the weight had only to be raised, in an average of twelve experiments, to a height of 5ft. 6ins. in order to break the rail; but when the thermometer rose from +3° to +8° C., then the weight had to be lifted for a fall of 7 feet 9 inches. Similar experiments conducted in 1860, showed that a difference of temperature from —4° to +5° centigrade was sufficient to greatly influence the height of fall necessary to break the rail. It is not unnatural to suppose that the particles of iron, after being worked at a heat and allowed to cool and set at a medium temperature, should, when that temperature is lowered, get into a state of mutual strain; or that any initial mutual strain should be thus intensified. The toys made of suddenly cooled glass, known as Prince Rupert's drops, are exaggerated instances of a similar action. The outside portions of a bar of whatever size, would evidently cool and consequently contract first of all. The inside portions would also at last cool, but, having kept the outside portions distended, when the inside does cool, it then becomes a question, to be determined by various circumstances, whether it would pull the outside shell into a state of compression, or whether the outside shell would draw the inside into fissures by tension. A somewhat similar explanation is given by Mr. Mallet of the rents caused in the interior of very massive forgings, and this state is probably always induced by the conditions of cooling in a small bar, but with, of course, a smaller range both as to size and temperature. In any case, it is apparent that a ductile, elastic material ought to be less affected by these doubtlessly complicated conditions of tensile and compressive strains. It is, therefore, probable that a hard, harsh, iron

would be more affected by frost than a soft ductile iron, and also that the breaking strength of both qualities would be less affected by cold than their extensibility. It is even by no means improbable, though the fact would be difficult, or at any rate very expensive, to prove, that the breaking weight, or the elastic limit, or both, of iron, is or are different for every degree of heat. A bar is perhaps cooled down in the rolling shed to the medium atmospheric temperature of, say, 52° Fahr. At a lower temperature, at a temperature for instance, of 32° Fahr., its static breaking weight is increased, but its power of elongation under stress is probably diminished. At, say, boiling point, its breaking strength is diminshed, but its power of elongation is increased. These remarks to some extent meet the results of Baudriment and Fairbairn. Unfortunately, Baudriment has not recorded the elongations, and his experiments were made on wires only one millimetre in diameter when at a temperature of 16° C. Dr. Fairbairn did find that the elongations of plates increased very closely with the temperature, but his experiments are not sufficient in number to be taken as conclusive; and, as Dr. Percy remarks, many more experiments are required on the action of frost on iron. If it could be shown, for instance, that the crystals of iron expand to different degrees in their different axes, this would probably, *per se*, meet the scarcely-to-be-doubted fact that iron is rendered brittle by frost. As the chain cables of a ship are alternately exposed to the utmost extremes of atmospheric temperature, this question is here of peculiar importance.

(To be Continued.)

Spoked and Disk Wheels.

From the Practical Mechanic's Journal, June, and July, 1864.

(Continued from page 86.)

M. Martin having been engaged to assist in conducting the experiments made under the direction of M. H. Bochet, mining engineer, the first deductions from which are inserted in the second part of the *Annales des Mines* for the year 1858, the whole of which treat of the friction of rolling; but the second part, which is not yet concluded, will treat of the variations of those resistances which the action of the air produces; and lastly, every thing which concerns the questions of velocity, specific pressure, of lubrication, &c., &c.

M. Bochet having been requested to give information on this interesting and no less important subject, prior to the entire publication of his extensive researches on those points which relate to the action of air upon revolving wheels, has addressed M. Martin in the following terms:—

"It appears from the numerous and exact experiments which I have made with a dynamometer, to determine the resistance which is offered to the rotation of the wheels, that this resistance is made up of three different forms, as follows:

"First. The resistance due to the friction of the journals of the axle in their bearings; secondly, The friction resulting from the rolling of the wheel on the rail; thirdly, The resistance of the atmosphere upon all the surfaces exposed during rotation.

"If we compare one with the other, the total resistance opposed to spoked wheels on the one hand, and on the other that opposed to disk wheels of the same diameter, breadth of tyre, and weight, all other things being the same, as well as the velocity of rotation, the total resistance opposed to disk wheels is always much less than that offered to spoked wheels.

"This diminished resistance offered to disk wheels, comparatively with that to which spoked wheels are subjected (the two first elements of resistance above mentioned being identical in each case), although of immense importance at a slow rate of revolution, becomes of still greater importance as the velocity of rotation is increased.

"At a speed 400 turns per minute, the resistance of the atmosphere upon disk wheels is not more than half of that offered to those with spokes (this fact was obtained from observations made upon the wheels of a truck, whose diameter was 1 metre). This difference of 1 to 2 is not a variation which is produced from the sum of the resistances encountered, but that of the atmospheric resistance only; for in both instances the resistance of the journals in their bearings and the rolling friction of the wheels upon the rails remain constant; but when the wheels have only a light weight to carry, and especially if the journals are well lubricated, the two last mentioned resistances are considerably lessened; and above all, at a high velocity, they are much less than generally supposed; so that at a high velocity of rotation, the entire resistance offered to the revolution of a disk wheel is rarely if ever increased beyond the half of that offered to a spoked wheel of similar dimensions. It is also well to add, that the small disturbance of the atmosphere by disk wheels when traveling at a high velocity, is a special advantage, independently of the diminished resistance to traction which results.

"It may also be stated, that as the load becomes greater, and the lubrication of the journals becomes inferior, the rate of revolution diminishes in an exactly corresponding proportion, the total resistance encountered by the wheels always reverting towards an equilibrium; that is to say, as the load and friction increase, the velocity of rotation decreases; and as the load and friction decrease, the velocity of rotation increases.

"H. Bochet"

These experiments were made with the special view to determine the results contained in the above letter. The same wheels were easily made either to present a disk or spokes to the resisting action of the atmosphere; for thin iron disks were prepared of a diameter equivalent to that of the spoked portion of the wheel; these were easily fixed on either side of the wheel, so as to make a plain disk surface; or when taken off, the spokes were left exposed as usual.

We think it well, in order to complete this interesting subject, to add some extracts from a paper by M. J. Poirée, Ingenieur des ponts et chaussés, published in the Transactions *de la Societe Ingenieurs Civile*. The experiments referred to as having been conducted by M. J.

Poirée, were carried out for the purpose of obtaining complete information upon every point which influences the traction of railway trains, such as the work in ascending inclines, as well as of passing over curves, the resistance to be overcome in the engine itself, its actual power, its consumption, the atmospheric resistance opposed to trains, &c., but strange to say, that the obstruction offered by this element to the rotation of the wheels, although it was inquired into, was not carried out so as to obtain any satisfactory results.

The atmospheric resistance opposed to various classes of trains may be stated approximately in the following order:—

To the traction of	ballast trains,	at	3k. 2·10	per tonne.
" "	goods trains,	at	4 1·2	"
" "	passenger trains,	at	7 7·10	"
" "	express trains,	at	10	"

These experiments were made with a Morin dynamometer, placed in the back part of the tender of the engine which drew the experimental train. The instrument was capable of measuring the opposing forces as high as 2300 kils.

An anemometer, destined to give the intensity and direction of the currents which acted upon the train, was placed above the tender. Lastly, watchmen placed on the line noted the natural and chief direction of the wind both before and after the train had passed.

In the whole of these experiments, the traction per tonne on the level varied from 3 kils. to 14 kils. The resistance of the atmosphere per square metre, as shown by the anemometer, varied from 0 to 57 kils. per square metre. In the last case, the train's lineal velocity was 20 metres per second, with a contrary wind. The influence of a side wind is very important. With a nearly constant velocity of travel 56 kilometres per hour, the load of 117 tonnes also remaining constant, the tractive force on a level was found on two different journeys to vary from 6k. 5 to 10k 5 per tonne.

In the first case, the wind acted upon the train precisely at an angle of 10° to its direction of travel; but in the second, it acted in opposition to it at 28°.

If the train is supposed to consist of six carriages, making a total weight of 80 tonnes, running at a speed of 12 metres per second, and acted upon contrary to its travel by the wind striking it at an angle of 45° with a velocity of 6 metres per second, it is found that the resistance due to the impact of the atmospheric currents upon the anterior portion of all the vehicles composing the train amounts to 3 kils. If the same wind, however, struck it in the direction of the trains motion, the resistance per ton due to the action of the atmospheric currents would only be 0k. 22. Upon a calm day, the resistance would only be 0k. 50.

These experiments have shown the wide differences which actually exist between the above numbers; but in this calculation neither the resistance that the atmosphere exercises upon the wheels, nor the friction of the wheels upon the rails, have been taken into account.

The Traction of Passenger Trains.—English engineers have adopt-

ed, and the authors of the "*Guide des Mecaniciens*" have recommended the following formula for this calculation:

$$R = 2k.72 + 0{,}094\,v + 0{,}00484\frac{NV^2}{T} \quad . \quad . \quad (11)$$

R being the resistance per train tonne.
V the velocity per hour, in kilometres.
T the total weight.
N the total train surface exposed to the direct resistance of air.
2k.72 represents the co-efficient of friction.
0094 proportional to the velocity, expresses the resistance due to concussions, undulations, and vibrations of the permanent way.
000484 proportional to the square of the velocity is the resistance of the wind.

This formula re-produces the numbers found in M. J. Poirée's experiments, making

N = 14 metres for train of carriages;
V = 7 metres for express trains.

M. Martin had recourse only to the preceding when calculating according to the indications given by the anemometer, the resistance per tonne drawn, due to the impact of the air upon the anterior portions of all the vehicles composing the train which was found to vary from 3k.00 to 0k.5, has proved that the total resistances are very much greater, but that he had not taken into account the resistance of the atmosphere upon the wheel, or the friction on the rails.

Comparative weight of Spoked and Disk Wheels.—If we compare one with the other wheels of 0m 92 in diameter, they will be found to have the following weights:

Wheels with single spokes, 133 kils.
" with double spokes, 181 "
" with disk centres, 130 "

and those of 0m 825 diameter—

Wheels with double spokes, 154 kils.
" with disk centres, 110 "

the main weight of spoked wheels will therefore be

$$133 + 181 + 154 = \frac{468}{3} = 156 \text{ kils.} \quad . \quad . \quad (12)$$

that of disk wheels—

$$130 + 110 = \frac{240}{2} = 120 \text{ kils.} \quad . \quad . \quad (13)$$

that is as 156 : 120, or as 15 : 10 . . . . (14)

Disk wheels beyond all doubt are destined in no long period to supersede those of the present construction: the employment of thin steel tyres will also be rendered capable, by reason of their being tempered

without fear of altering in form, they being supported continuously in their interior.

The doubts which have been entertained relatively to the solidity of these tyres need no longer exist, according to the report of the experiments carried out under a commission composed of M. Combes, Laieux, and M. Couche.

This commission has proposed to reduce to one-half the thickness of the tyres, which will enable them to be cast in steel with facility. The conclusions of this report have been adopted by the *Commission centrale des machines à vapeur.* It will be easily perceived how advantageous it becomes to employ steel tyres, which will not quickly wear out, for when their thickness is reduced to a centimetre, (·3937 inches) as far as strength goes they will be equal to iron tyres of 20 millimetres (·7860 inches) in thickness.

According to these conclusions the weight will be, for an axle with wheels attached, of a diameter = 1m 03 = (40·462 inches), and with straight spokes, 815 kilogrammes = (396·6 lbs.); and for the same axle with disk wheels, of the same diameter and steel tyres, 0m 025 = (984·275 inches) in thickness, the total weight would be 645 kilogrammes = (1422·096 lbs.)

On the Amount of Dust lifted by the Wheels of Railway Carriages and Wagons when running.—It will not be contested, after what has been stated, but that disk wheels are far superior in every way to those made with spokes, and, considering the grave objections judiciously raised against the latter, it becomes a desirable matter that these should be transformed into disk wheels, which change may be effected in two different ways, either of which will permit the same wheels as at present existing to partake of the advantages experimentally determined as belonging to disk wheels for railway vehicles. One mode of effecting this change consists in placing on each side or face of the wheel a thin disk of sheet iron or wood; these may be firmly held in place by bolts passing through both at intervals; or, instead of this, it may be preferable to fill the space between the spokes by solid blocks of wood, held to the spokes by means of iron studs. This mode of effecting the jointing is preferable to the use of bolts and nuts, which are usually employed for approximately similar purposes, from the fact that these frequently fall out in traveling, becoming loosened by the centrifugal action of the wheel, and shaken out of their places by the trepidation of the permanent way as well as the train.

The dust raised does not only injure expensive carriages, but it penetrates the axle boxes, and frequently commits serious injury on the journals and their bearings; it is also excessively annoying to passengers.

The Cost and Economy of Disk Wheels.—As to first cost, disk wheels would be about the same as those with spokes, and taking into account the economy in traction resulting from their employment, which, putting it down at minimum, cannot but be of immense importance, as may easily be seen by calculating the number of trips run by each pair of wheels during a given period.

The disk wheels which have served for the above comparison with the spoked ones, were furnished by "*la Societè des forges de la Providence.*"

We have now brought to a close these comparisons between "spoked and disk wheels," and trust that some of our English engineers may ere long bestir themselves toward adopting an improvement which it appears they have been too slow in discovering the advantages of. Disk wheels of cast iron have long been in use upon the American railways, and although it appears likely that these deductions of M. Martin were made with regard to wrought iron disks, still it is very probable that cast iron will ultimately be the material adopted for the purpose. Disk wheels, it is true, have been in use in England for some considerable length of time, the disk being formed of wooden segments under a system patented by Mr. Beattie, of the London and South Western Railway, and the employment of which is largely extending; we have also frequently seen trucks and wagons with wooden disk wheels on the Bristol and Exeter line, but we do not believe that in either case they were adopted for the reason of diminishing resistance to traction, or any of the other advantages that are now shown to belong to them.

We do not confine these remarks to the wheels of carriages, vans, and wagons only—they are equally applicable to those of the locomotive itself, and if the change proposed be ever adopted, it seems very probable that the not unfrequent necessity of mending broken drivers may be quickly obviated. It has long been seen that existing spoked driving wheels are not at all suited for the increasing traffic on our railways; many of them are unable to endure the strain of a modern train. They were all very well and good at their birth; then the weight of the train was in proportion to their rigidity, but now-a-days we see ordinary trains four and five times the weight they were ten or a dozen years ago—still we have the same engines and the same wheels set to do twice or thrice their former amount of work. In a few cases, however, new wheels of larger scantling have recently been introduced. This certainly is anything but fair, and the requirements of the case must be met somehow or other. Let us see how this shall best be done. Not very long ago we were witness to a miserable blunder committed by the locomotive superintendent on a certain English line of railway, and on whom the proceeding reflects anything but credit, for, had the results which were likely to follow been seriously weighed beforehand, we can scarcely venture to believe that such a gross mistake would have been perpetrated. The facts of the case are the following:—The company had a limited number of engines, most of which were made some thirteen or fourteen years ago, at a period when the trains they had to draw were seldom more than one-half the weight and number of what they now are, still the same engines were expected and set to do all this extra amount of work; the consequence was, that from the additional number of journeys

which they had to run, say nothing about the amount of slipping of the driving wheels, caused by the heavy load, the tyres were in a comparatively short time, loosened, and worn out—so much so, that the annual expenses of repairs were largely increased by this item, at the same time much loss was suffered by the engine itself having to lay by the time during which the tyres were being turned up afresh, or re-tyred, as the case might be. Thus so much capital was doing nothing. How to obviate these recurrences was the problem to be solved, and in what way was this done? Heavy, that is *thick* tyres had all along been used; but it was thought that if these were increased in thickness, and put on the wheels tighter than before, that the loosening would be avoided, and that the wheels would run much longer without being re-tyred; but we shall presently see if this was practically the result. Tyres of a monstrous thickness were ordered (we regret we cannot state the exact dimensions, having missed our memoranda respecting them); they were bored of little less interior diameter than had been the practice before (with thinner tyres), and put upon wheels about 6 feet in diameter; the consequence was, that after lifting the first wheel out of the cooling cistern, the spokes, straight beforehand, were found, each one, to be curved to no slight amount. This, however, was passed over quite lightly, as if merely nothing of consequence, and so several sets of drivers were similarly served. Some of them were, however, soon set to the crucial test. They had not been running many weeks before the spokes became still more curved, and the wheels loose in the tyres, the heavy weight of which, tumbling about on the wheels when running, smashed them through and through every division between every two adjoining spokes, and several of the spokes were also broken; in fact, many of the wheels, when the tyres were taken off, fairly fell in pieces—and thus the last error was worse than the first. All these alterations made with the view of saving expenditure! Now, we venture to state, that had due consideration been given to the results which were likely to follow from such a proceeding, no such course could have been entered upon by any person understanding what he was about. The results were predicted by others, but no heed was taken; nevertheless it was not long afterwards discovered that the despised prophecy was in reality too true. We have cited this instance at some length, mainly because we think it right these things should be made public, in order that others may be warned from running into such or similar errors. We think it unnecessary to enter at further length on this subject at present, and conclude by recommending urgent attention on the part of British engineers to the advantages resulting from the employment of disk wheels for railway purposes; still it must be remembered that the results which followed from above-named proceedings would have occurred with a wheel of any make, provided the tyre was constructed of sufficient section and put on tight enough to crush the interior, which must have given way in some shape.

Paper on Foundations for Chimneys, Blast Furnaces, Heavy Machinery, &c. By JEREMIAH HEAD, Engineer, Stockton-on-Tees.

From the London Practical Mechanic's Journal, July, 1864.

Large works, such as blast furnaces, rolling mills, &c., are generally sought to be erected, other things being equal, on the banks of a navigable river. The advantage of a private wharf is the reason for preference given to such sites.

The Clyde, Tyne, Wear, Tees, Humber, Thames, Avon, and Mersey, have each of them their banks studded more or less with manufactories. Most of these works have erections requiring sound and immovable foundations, as, for instance, tall chimneys, furnaces, and heavy machinery, and it frequently happens that the banks of rivers afford only the very worst of foundations.

Fig. 1.

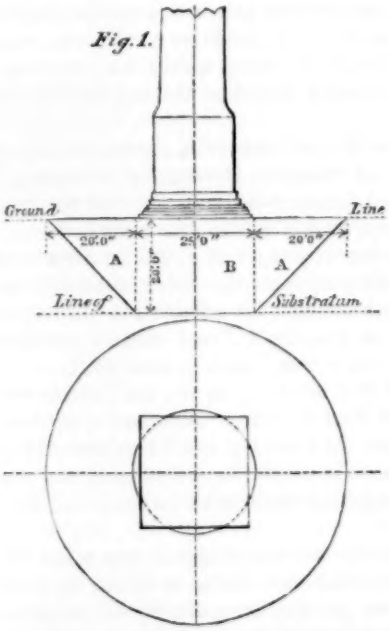
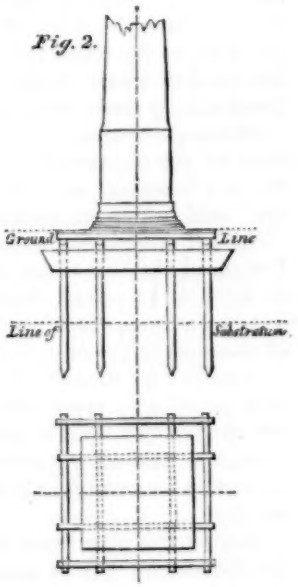


Fig. 2.



Take the Tees for instance, the heart of the Cleveland district, and the banks of which are becoming rapidly disposed of as sites for various iron works. The south bank consists mostly of an extensive flat marsh, in many parts as much as four feet below the level of spring tides, which are kept in check by an artificial bank. A section of the strata gives usually a crust of four or five feet of alluvial clay, in all probability gradually deposited by floods from the river. Below the clay we have from five feet upwards of peat, containing trunks of trees which have known other days. Below that again, down to from twenty to thirty feet, we find a soft bluish silt, incapable of affording

a foundation. At twenty to thirty feet there is often either good clay or a hard sand, either of which are sufficient to satisfy the most fastidious engineer.

It is then from this depth, of from twenty to thirty feet, that we must support our fabric.

There are two ways commonly in vogue for obtaining a foundation from this substratum. The one is to excavate down to it and fill up entirely or partly with solid concrete, (see fig. 1,) the other is to pile, (see fig. 2.)

Both these methods are attended with disadvantages. In the former, the ground in the above named locality is, as vulgarly termed, "rotten" to such a degree, that a batter of one to one is necessary to prevent land slips, and should it be in the winter season, even this is insufficient. This is supposing that a foundation of, say not less than twenty-five feet diameter upon the substratum as for a chimney is necessary. If that substratum be twenty feet below the surface, the excavation must be $20 + 20 + 25$ or 65 feet diameter at the top, which gives about $3\frac{1}{2}$ times the solid content of what would be necessary if we could make the excavation twenty-five feet at the top and dig perpendicularly down without batter.

If the excavation is filled up with solid concrete, there is the drawback of the expense of $3\frac{1}{2}$ times as much as is actually necessary, as the overhanging parts, A A, (fig. 1,) take a bearing only on the "rotten" soil, which we suppose at starting is not to be trusted.

And if we build a solid cylinder of concrete, equal in area to the bottom of the excavation, and with perpendicular sides, then soil must be filled in all round, doubling the account for excavation of the parts A A, or in figures making it $3\frac{1}{2}$ less, $1 = 2\frac{1}{2} \times 2 = 5$ times the content of the solid cylinder B, *i.e.* five times more than is necessary.

And yet we cannot get the 25 feet circle upon the solid substratum without all this extra labor upon this system. Another objection is the form of this kind of foundation. According to all mechanical principles the base of any structure should be the broadest part, the work gradually narrowing as it ascends, like a basin with its open side downwards.

But on this plan from the difficulty in excavating in any other way, the foundation has to assume the form of a basin with its open side upwards, and hence, should there be any irregularity of weight, or position of the superstructure, there could hardly be a worse form to resist a tendency to roll. Piling, which is the method of securing a good foundation generally preferred in such cases, is liable to the following objections, viz:—

1st—It is very expensive, and if the piles are required of a greater length than about 35 feet, they are often difficult to obtain at any price.

2d—If the pile heads project above the surface they are liable to decay at the surface line. If the concrete, usually placed about their heads and crown-trees to keep them in position, be upon rotton ground,

as in the case assumed, it frequently subsides, tearing itself away from the crown-trees, and leaving the pile heads liable to decay.

If flues, kilns, or stoves pass near them, the crown-trees are apt to get charred or burned, letting down the superstructures. A case of several air stoves, for blast furnaces, being rebuilt from this cause, has lately come under the writer's notice.

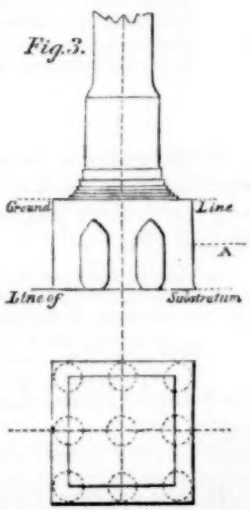
To obviate these difficulties, the writer would submit a plan which he has patented, and believes to be new, having never seen or heard of it before.

Although it is not practicable to dig down twenty feet in "rotten" ground, to obtain a circle of 25 feet diameter, and upwards, without allowing so much batter as to increase the cost of excavation most materially; it has been found by experiment perfectly possible to sink a well of, say 6 to 8 feet diameter, perpendicularly down to that depth, without any fear of land slips.

The circle is so small that the surrounding strata, pressing together, feel the resistance of the arch form, and neutralize one another.

It is proposed, then, to take advantage of this fact, to make the foundations of a series of wells, dug one by one, in order to keep the work perfectly under control, and filled up with concrete or slag walling, *i.e.*, rough rubble walling, formed with lumps of blast furnace slag, a material greatly abounding in the above locality. If a greater depth than 24 feet, or if the nature of the stratum renders it necessary at a less depth, it is recommended to make the wells square, with two planks at each corner placed vertically, each taking a side of the corner; these planks to be strutted at intervals in the depth; they are to be hammered down as the work proceeds, and drawn out as the concrete rises. An immediate stage, with an extra hand upon it, will lift all the soil down to a depth of, say, 12 feet, below which a jack-roll and buckets will be necessary. Near the bottom it is recommended to widen the base of the excavation slightly, in order to incease the footing of the concrete pillar. It is obvious, that by this plan, instead of the foundation being firmest under the centre of the chimney, or other structure, forming a pivot upon which it might tend to work, the concrete pillars may be disposed more at the corners, forming a spreading base which is the desired end to obtain.

Fig. 3.



The platform on which to commence building would, on this system, be formed also of concrete or slag walling, say, five feet thick, and moulded in the following manner:—The wells having been filled up to A, fig. 3, within, say, 6 or 8 feet of the base line of the structure,

they would then be widened out, working from a round section into a square one, if a round section has been adopted. The intermediate soil must then be cut into the form of a soffit of a ground arch, and the concrete made up to the ground line in the form shown in fig. 3.

This foundation, besides the advantages of easy execution, wide spread base, and minimum cost, possesses the very great one of being formed only of imperishable materials.

We will now compare the costs of foundations on the three different plans for a chimney, say 500 tons in weight where the substratum was 20 feet deep.

1st. *Solid Concrete Foundation*, 25 feet diameter at bottom, 65 feet at top, and 20 feet deep—(fig. 1.)

Excavation, 1272 cubic yards, at 7½d,	£ 39 15 0
Cylinder of concrete, 25 feet diameter and 20 feet deep, 363 cubic yards, at 3s. 6d.,	63 10 6
1272—363=909 cubic yards, at 7½d. for filling up round concrete,	28 0 0
Pumping, say,	20 0 0
	£ 151 5 6

2d. *Pile Foundation*—(fig. 2.)

16 piles, 30 feet long, at 6s. each driver,	£ 48 0 0
Crown-trees, 260 feet, at 2s,	26 0 0
3 planking overtops, 1056 feet, at 4d,	17 12 0
Excavation, 274 yards, at 7½d,	8 11 3
Concrete, 182 yards, at 3s. 6d.,	31 17 0
Spikes,	0 10 0
Use of pile driver, say,	3 0 0
	£ 135 10 3

If the under crown-trees alone were whole timbers, and the upper ones half-timbers, 6 inches apart, thus forming the platform and doing away with three inches planking, a better job would be made, but the cost, £ 144.

3d. *Arched Foundation*—(fig. 3.)

9 wells, 6 feet diameter, each 21 cubic yards, platform 30 feet square and 5 feet deep, say 300 yards of excavation, at 7½d,	£ 9 7 6
300 yards of concrete or slag walling,	52 10 0
Pumping, say at 3s. 6d.,	18 0 0
Labor for jack-roll, say,	5 0 0
	£ 84 17 6

It thus appears that the arched system, besides the advantages of better mechanical form and imperishability, would cost in the case assumed little more than half as much as either of the usual methods.

In some cases it may be advisable to carry out the same principle by excavating in deep trenches, either in straight lines or in circles, and filling up and arching over, as explained above. An annular excavation, with, perhaps, a single massive pillar in the middle, is particularly recommended where there is not room for an extended base on pillars, as, for instance, two or more blast furnaces close together.

The excavation while in progress is strutted from side to side to prevent it falling in, and is filled up with concrete or slag walling as it advances.

MECHANICS, PHYSICS, AND CHEMISTRY.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

[Report of Chief Engineer, March 8, 1864.]

From the Lond. Practical Mechanic's Journal, April, 1864.

This report was a double one, embracing the month of January as well as that of February—since the annual meeting having been held upon the same day as the last ordinary monthly one of the Executive Committee, the consideration of engineering matters had then been postponed till the next meeting. Of this report the following is an abstract:—

"During the last two months, *i.e.*, from January 1st to February 19th, inclusive, the ordinary visits of inspection have been made; 2 boilers tested by hydraulic pressure, and 307 defects discovered in the boilers examined, 10 of which were dangerous. Details of these defects will be found in the following statement:—

Furnaces out of shape 5; fractures 8 (1 dangerous); blistered plates 3 (1 dangerous); corrosion 45 (5 dangerous); grooving-internal 6; feed apparatus out of order, 1; water-gauges ditto, 45 (2 dangerous); blow out apparatus ditto, 43; fusible plugs ditto, 1; safety-valves ditto 5; pressure-gauges ditto, 13 (1 dangerous); boilers without glass water-gauges, 14; without pressure-gauges 4; without blow-out apparatus 24; without feed back-pressure valves, 83; cases of over pressure, 5; deficiency of water, 2; in all 307.

Of some of the defects enumerated above a few particulars may be given:

Blistered Plates.—A blister, measuring 24 inches by 12 inches, and three-sixteenths of an inch in thickness, has been met with inside an internally-fired boiler, 8 feet in diameter, and working at a pressure of 45 lbs. on the square inch.

Corrosion—Internal.—In two boilers, though only eighteen months old, the plates, which had been seven-sixteenths of an inch thick originally, were found to be eaten half through, while the rivet heads also were attacked. The plates were honey-combed, being affected more severely at the furnace than at the back end of the boiler. The feed-water used was drawn from a well.

In another boiler which was of Lancashire construction, and fed from the Rochdale Canal, the furnace crowns presented a spongy appearance, and channeling had set in at the transverse seams, which is unusual in internal flue tubes; also the rivet heads and angle irons were attacked, as well as the whole boiler generally. Some of the channels were cut half through the plate, while many of the other indentations exceeded this.

Internal corrosion has been successfully arrested, in many cases by the use of carbonate of soda, introduced in small and frequent quantities with the feed-water.

Corrosion—External.—Two wagon boilers were found to be so corroded along the brick-work seating, that holes could be scraped through the plates.

Two Lancashire boilers, set upon side walls, though free from leakage at the seams of rivets, were found on "thorough" examination, to be seriously corroded at the seating, in consequence of water, arising from the discharge of the safety valves, &c., being allowed to fall upon them.

Two other instances occurred to Lancashire boilers, in consequence of their being set upon mid-feather walls. It is always recommended that a portion of this wall should be removed, by way of preparation for a "thorough" examination, otherwise the true condition of the plates cannot be ascertained. In this instance, this recommendation was not complied with in time for the inspector's examination, so that he was unable to see the amount of corrosion going on. But upon this recommendation being subsequently adopted, the plates were found to be so dangerously eaten away, that the owner at once condemned the boiler, and our inspector found it removed on his next visit. In the other case, the plates over the mid-feather, in the neighborhood of the transverse seams, were found to be reduced in places to one-eighth of an inch in thickness.

Four cases of external corrosion occurred at the bottom of the shells of internally-fired tubular boilers, on account of the unequal expansion of the metal, consequent on the imperfect circulation of the water. The constant straining induced by this unequal expansion had produced leakage at the transverse seams, and thus corroded the plates. In such cases caulking is of little use. In many boilers of this class under inspection, the edge of the overlap has been so repeatedly trimmed and recaulked, that the margin beyond the rivets has been at length all cut away. The only radical cure is to maintain the whole of the shell at an equal temperature, and this, in many cases, has been found to be sufficiently accomplished by carrying a return flue under the bottom of the boiler.

Two other cases arose from leakage at the tube ends of multitubular boilers, from which the plates at the bottom of the combustion chambers were nearly eaten through.

Pressure gauges out of order.—One has been met with which only indicated 14 lbs. as the blowing off pressure, while it proved to be as much as 35 lbs. This shows the importance of all boilers being fitted with suitable taps, in order that the inspector may apply his indicator, so as to ascertain the actual pressure, and check the accuracy of the steam-gauge at each of his visits.

Explosions.—An explosion occurred on Wednesday, December 30, 1863, which was not reported in time to take its place in last year's list, so that the number of explosions for the year 1863, which came under the notice of the Association, will now stand as 48; while the

number of persons killed and injured will remain unaltered, since, happily, no one was hurt by the explosion just referred to.

The boiler in question, which was not under the care of this Association, was of plain cylindrical egg-ended construction, and externally fired.

The cause of the explosion was simply neglect on the part of the engine-tender, who allowed the boiler to get short of water. In consequence of this the plates over the fire became red-hot, and so weakened, that rupture ensued. Though the rent was confined to the bottom of the shell immediately over the fire, the boiler was blown from its seat and thrown upon the ground, bottom upwards; while another, working alongside, was also dislodged, and thrown to a greater distance than the one that burst.

The engine-tender was prosecuted for neglect, and sentenced to a month's imprisonment with hard labor.

Of No. 47 explosion, which occurred on December 31, 1863, some additional particulars of interest have been obtained since the issue of the last report.

The boiler referred to was No. 1 of a series of three, connected together and working side by side, all of them being of plain cylindrical egg-ended construction, and externally-fired. The furnace end was rent into five or six pieces, and the fragments scattered, some of them being blown to a considerable height, while the remaining portion of the shell was thrown from its original position, and the seating altogether destroyed.

It is of interest to note that the adjoining boiler was also dislodged, and battered in at its side for nearly half its length, since it affords an illustration, by no means the first that has been met with, of the manner in which, what may be termed compound explosions arise, and which it appears spring from externally-fired boilers only,

From the commencement of the present year up to February 19, five explosions have to be reported, from which fifteen lives have been lost, and also twelve persons injured, some of them very severely. Not one of the boilers in question was under the charge of this Association.

These explosions have not all been personally investigated, and when such is the case, the Association is frequently indebted, as in the present instance, to engineers, who, residing in the respective localities of the explosions, and having the opportunity of making examinations, kindly furnish the detailed reports and sketches.

No. 1 Explosion.—The boiler in question worked at a colliery, and was not under the charge of this Association. It was one of a series of two working side by side, both of them being of balloon or haystack shape, 13 feet in diameter, and having their safety-valves loaded to 14 lbs. per square inch, although usually the engine worked with steam at a pressure of 5 lbs. only.

The explosion took place on a Monday morning, the boiler and the engine having been at rest on the previous day. The weather was frosty at the time, and the feed pipe, as was found upon examination

after the explosion, became choked with ice. This cut off the supply to the boiler, and it consequently exploded from shortness of water.

The boiler was rent into seven pieces, the steam pipes broken up, a portion of the engine-house thrown down, and the top of the chimney-stack damaged. The fragments of the shell and steam pipes, as well as the debris of the building, were scattered to a considerable distance, but although there were one hundred and twenty men in the vicinity of the boiler, and many of the flying bricks struck the roof of an adjacent cottage, in which its residents were sleeping at the time, yet happily no one was injured.

No. 2 Explosion, which resulted in the death of one man, occurred to a boiler not under the inspection of this Association, and employed driving an engine attached in a limestone pit.

It appears that the boiler was 6 years old and one of a series of two, working side by side, and connected together, both of them being of plain cylindrical egg-ended construction, and externally-fired. The length of the one in question was 31 feet 8 inches, the diameter 7 feet and the original thickness of the plates from three-eighths to seven-sixteenths of an inch; while the pressure to which the safety-valve was loaded was 48 lbs. This, however, would be exceeded whenever the steam blew off at all briskly, and, since there was no steam-gauge, the exact pressure would not be known. Excepting the omission of the steam pressure-gauge, which, though too common, should never occur, the boiler was fitted with such mountings as are usual to its class, and might have been safely worked at a pressure of 50 lbs. as long as it was in good condition.

Such, however, was not the case. Leakage had been going on for some time on the top of the boiler, both at the seams of rivets and at the joint of the feed-pipe, which was not riveted to the shell, as it should have been, but merely bolted. The water from this leakage ran down and corroded both sides of the boiler, though it affected the right hand one more especially, the plates of which it reduced in thickness to nearly one-sixteenth of an inch, for a length of about 8 feet horizontally, just at a part concealed from view by the brick-work in which the boiler was set. At this weakened part, which was below water-line, the shell rent in the first instance, and then tore from end to end, opening out nearly flat and separating into three pieces.

All these fragments were thrown from their original position, one of them to a distance of a few feet only, and the other two about 40 yards across a colliery railway. The seating of the boiler was torn up, the engine-house shattered and unroofed, and the debris scattered in every direction. The man who lost his life—though standing on the opposite side of a canal to that on which the explosion occurred, and at a distance of about 40 yards—was struck by a brick shot away by the percussive action of the steam, and died shortly afterwards from the effects of the blow; while the engine-driver and another man in the engine-house escaped comparatively uninjured.

It appears that there was plenty of water in the boiler at the time that it burst, and there is no need of the supposition, that any unusual

pressure of steam existed, since the fact of the plates being thinned by corrosion, as they were found to be, is quite sufficient by itself to account for the explosion.

At the inquest, the owner of the boiler, through his solicitor, attempted to prove the engine-man at fault, and show that the explosion was due to excessive pressure of steam, consequent on his negligence, and not to the defective condition of the boiler. The jury, however, returned a verdict of manslaughter against the proprietor, as well as the superintending engineer of the works, both of whom it appears had been apprized, more than once, of the leakage which was going on, and their attention called to the necessity of further examination. The verdict of the jury was, however, shortly after set aside on account of informality.

No. 3 Explosion, from which one person was injured, but happily no one killed, occurred to a boiler not employed in the generation of steam power, but for heating and ventilating a public building. This boiler was not under the inspection of this Association.

It will be seen by the date, (January 11th,) that this explosion took place during the severe frost at the commencement of the year; while two other explosions happened to household boilers, just about the same time, by which one person was killed and three others injured. It was through the frost that these explosions arose. The water in the pipes froze and thus the outlets became choked, and the steam pressure bottled up, so that on the fire being maintained for a sufficient time explosion became inevitable from accumulated pressure. As long as the water in these boilers retains its liquid state its own inlet column forms a natural safety-valve, but since this action is entirely arrested on the occurrence of frost, every boiler employed for heating and ventilating public buildings, as well as those for household purposes, should be fitted with an efficient mechanical safety-valve, which would remain unaffected by changes of temperature. The importance of this with regard to domestic boilers is too apt to be overlooked, but I have known neglect of this precaution to result in fatal consequences in more than one instance.

No. 5 Explosion was of a very serious character, resulting, not only in considerable damage to property, but also in the death of thirteen persons, and in injury, which in some cases was very severe, to ten others. It took place at an iron works, the boilers of which were not under the charge of this Association. The scene of the explosion has been personally visited, and the cause investigated, but, since the coroner's inquiry is still pending, the publication of the particulars is deferred to the next monthly report.

P.S.—Since the above report was closed, which only extends, as stated, to the 19th of February last, another most serious explosion, resulting in the death of ten persons, as well as injury to seventeen others, has occurred at an iron-works to a boiler not under the charge of this Association. The scene of the catastrophe has been personally visited, and the particulars will be given in the monthly report for March to which the explosion belongs.

It will be observed that already twenty-five persons have been killed since the commencement of the year by the six explosions which have occurred.

On Alloys Containing Tungsten.

From the London Mechanics' Magazine, March, 1864.

Caron has instituted a series of experiments by order of the French Minister of War to determine the influence produced by tungsten upon the qualities of bronze, cast iron, and steel. The only French mine from which tungsten is obtained in quantity belongs to M. Dubreuil, and is situated at Puy-les-Viques near St. Leonard, in the department of the Haute Vienne. The wolf-ram is here pulverized and roasted to separate sulphur and arsenic. In this state it could be sold at 2f·60 the kilogramme, but it is better to reduce it to the metallic state by heating with enough carbon to reduce the metals. The crude tungsten contains iron, manganese, and carbon, and is now sold at 3f·75 the kilogramme, a price which will probably be still further reduced. Tungsten as thus prepared was found incapable of forming true alloys with copper, tin, and gun-metal, the latter becoming less homogeneous and less tenacious than ordinary bronze, although harder. The addition of tungsten to cast iron was found on the contrary to increase both the hardness and tenacity in about the same ratio with the quantity of tungsten added. Even a small percentage, not exceeding one per cent. of tungsten, was found to exert a marked influence, the grain of the iron becoming regular, fine and grayish, and the fracture showing great homogeneity. The addition of tungsten, to steel was found always to increase both its hardness and its tenacity. The alloy exhibits a peculiar fracture with a brilliant lustre, so that tungsten steel is easily recognised by a practised eye. Poor steel requires more tungsten than steel of good quality. A good cement steel alloyed with 5 per 100 of tungsten gave a steel of excessive hardness which, however, forged very well, though it required much more force than ordinary steel. After tempering, it acquired a hardness comparable only to that of the hard white cast iron. A gun barrel made of steel, which on analysis proved to contain tungsten, withstood the explosion of a charge 0m.6 in length and weighing 150 grammes, with 5 balls of lead weighing 135 grammes. The author succeeded in obtaining a steel of similar quality by fusing together in an earthen crucible at a very high temperature, 200 grammes of highly carburetted steel, 800 grammes of good iron, and 20 grammes of tungsten. In conclusion, Caron confidently recommends the employment of tungsten to improve the qualities of steel, and shows that with tungsten at 3f·70 the kilogramme, the price of steel would be increased by only 7 or 8 francs the 100 kilogrammes.—*Ann. de Chimie.*

On the Supposed Nature of Air prior to the Discovery of Oxygen.

By GEORGE F. RODWELL, F.C.S.

From the London Chemical News, No. 207.

(Continued from page 119.)

3. *Discovery of the Pressure of the Air.*—Some years after the publication of the "Mathematical Dialogues," a work "On Motion" appeared, the ingenuity of which so struck Galileo, that he sought the acquaintance of its author, Torricelli. Torricelli* became the pupil of Galileo, but he was not long destined to study under so great a master, for three months after their first acquaintance Galileo died; not, however, before he had imparted to his pupil much of the knowledge acquired during a most active scientific life, and had exhorted him to investigate the cause of several unexplained phenomena, among others that of the limited height to which water rises in a pump.

In making some experiments with a view of arriving at the cause of this latter phenomenon, Torricelli filled a tube, closed at one end, four feet long and a quarter of an inch in diameter, with the heaviest liquid he could find, viz: mercury, and inverted it in a basin of the same liquid; instead of the mercury remaining suspended in the tube, as water would have done, it sank to such an extent that its height above the surface of the mercury in the basin was about 30 inches. Torricelli concluded that this result was produced by the air pressing on the surface of the mercury, but other philosophers preferred to attribute it to Nature's horror of a vacuum.

In 1644, an account of the experiment was sent to Mersennus,† who conceived that there were small hooks in the atmosphere which supported the column of mercury in the tube.

Pascal‡ was not in Paris when Torricelli's experiment was first made known there, and did not hear of it till 1646. He immediately made some experiments on the subject, and published an account of them in 1647, in a treatise entitled "New Experiments concerning the Vacuum," from which we select the following:

A glass syringe with an accurately fitting piston was placed entirely in water, the open end closed by the finger, and the piston raised. An empty space was apparent in the syringe, and the finger was strongly pressed against the glass, but by increasing the magnitude of the empty space, no greater pressure was felt by the finger. When the closed end was opened, water immediately filled the empty space.

A glass tube 46 feet long, open at one end and sealed hermetically at the other, was filled with red wine, and inverted, the open end being placed in a vessel of water. The column of wine sank till it remained at the height of 32 feet§ above the surface of the water. When the tube was inclined, so that no part of it was more than 32 feet above the water, the wine rose and entirely filled the tube.

A syringe was placed in a vertical position, and its open end plunged in mercury; on raising the piston, mercury was observed to follow it,

* Born, 1608. Died, 1647.
† Born, 1588. Died, 1648.
‡ Born, 1623. Died, 1662.
§ A foot = 1 foot 1·125 inch.

until a height of 2 feet 3 inches had been attained, beyond which, however much the piston might be raised, the mercury refused to ascend.

From these and several other experiments, Pascal declares it to be his belief that the space above the mercury in Torricelli's experiment is not full of either ordinary air or that air which some philosophers assert is inclosed in the pores of all bodies, or of air remaining by chance between the liquid and the glass, nor does it contain vapor of mercury, or that subtle air said by some to be mixed with ordinary air. It is veritably void and destitute of all matter.

These views were combated by Father Noel, a Jesuit, who believed that the space above the mercury was only an apparent void. Noel affirmed that just as that which we call blood is a mixture of bile, phlegm, "melancolie," and of blood, so that which we call air is a mixture of fire, air, water, and earth. The fire and air, being more subtle than the water and earth, can penetrate glass, and the space which Pascal had stated to be absolutely vacuous he believed to be filled with a pure air, compelled to separate from its grosser part by the weight of the column of mercury, which dragged it through the pores of the glass. These objections were answered by Pascal, and their fallacy proved beyond dispute.

In the "New Experiments concerning the Vacuum," Pascal had given no actual proof that the suspension of the mercury was due to the pressure of the air, but soon after the publication of that treatise he thought of an experiment which would settle the matter definitely one way or the other; but as it was impossible to try it in Paris, he wrote to his friend Perier, who resided in Auvergne, and begged him to make the experiment.

In his letter to Perier, which was written on November 15, 1647,* he says he cannot believe that Nature, which is not animate or sensible, can be susceptible of horror, because passions presuppose a soul capable of sharing them. He believes that all the phenomena attributed to Nature's horror of a vacuum, are caused by the pressure of the air, and has devised an experiment which must decide the matter. "This is to make the ordinary experiment of a vacuum, several times in the same day, in the same tube, with the same quicksilver, sometimes at the bottom, and sometimes on the summit of a mountain, elevated at least 500 or 600 toises,† in order to prove if the height of the quicksilver suspended in the tube will be equal or different in these two situations. You undoubtedly already see that this experiment is decisive of the question, and that if it happens that the height of the quicksilver is less on the summit than at the bottom of the mountain (as I have many reasons for believing, although all those who have thought on this matter are against this opinion), it will follow necessarily that the weight and pressure of the air is the only cause of this suspension of quicksilver, and not the horror of a vacuum, since it is certain that there is more air which presses on the foot of the mountain than on its summit."

* See "Récit de la Grande Experience de l'Equilibre des Liqueurs."

† A toise = 6·39459 feet.

Pascal suggested that the experiment should be made on the summit of the Puy de Dôme, a mountain in Auvergne; but Perier was at Moulins when the letter reached him, and was unable to make the trial till the following September, more than eleven months after the receipt of the letter.

On September 19, 1648, Perier provided himself with six livres of well purified mercury, and two glass tubes, exactly similar in every respect, 4 feet long, and closed at one end. The tubes were filled with mercury, and inverted in the same vessel, at the base of the Puy de Dôme: the height of the column in each tube was found to be absolutely the same, viz: 26 inches 3½ lines* above the level of mercury in the vessel. One of the tubes was left at the bottom of the mountain, the height of the mercury within it being marked, and a man appointed to watch if it varied. Perier carried the other tube and part of the same mercury to the summit of the Puy elevated about 500 toises above its base, and on repeating the experiment, the height of the column of mercury was found to be only 23 inches 2 lines; "ce qui," writes Perier, "nous ravit tous d'admiration et d'étonnement." The experiment was repeated five separate times in different parts of the summit of the mountain, with the same result.

During the descent the tube was filled and inverted; at Lafon de l'Arbre: the height of the mercury was 25 inches. When the experiment was repeated at the bottom of the mountain in the same tube, the mercury was found to remain suspended at the original height of 26 inches 3½ lines, and the column in the tube which had been left there also stood at that height. The following day Perier repeated the experiment at the top and bottom of Notre Dame de Clermont, and found a difference in the height of the mercury amounting to 2 lines.

The different heights of the column of mercury at the various elevations at which Perier had made the experiment were as follows:

At Clermont,	.	.	.	.	26	inches	3½	lines.
7 toises above Clermont,	.	.	.	.	26	"	3	"
27 " " "	.	.	.	.	26	"	1	"
About 150† "	.	.	.	.	25	"	0	"
" 500 "	.	.	.	.	23	"	2	"

These results were communicated to Pascal in a letter dated September 22, 1648, and they fully confirmed the views which he had previously held.

The Puy de Dôme experiment proved indubitably that the suspension of the mercury in Torricelli's experiment was due to the pressure of the air, and not to Nature's horror of a vacuum. Hitherto, not only Torricelli's experiment, but all the phenomena which were at that time attributed to Nature's horror of a vacuum, could be explained equally well by this theory, or by that of the pressure of the air; and when Torricelli first made known his experiment, men pre-

* 12 lines = 1 inch.

† From the height at which the mercury stood Lafon must be at least 200 toises above Clermont.

ferred to explain it by Aristotle's theory, which had been received for so many centuries, rather than to accept a new explanation, of the truth of which they had no absolute proof. But the Puy de Dôme experiment supplied the absolute proof which was wanting, for (as Pascal says in his first letter to Perier) we know that more air presses on the bottom of a mountain than on its summit, but no one can affirm that Nature has a greater horror of a vacuum at the level of the sea than she has on the summit of a mountain; if, therefore, the column of mercury is found to sink the higher the tube is carried, proof is at once afforded of which theory is to be accepted, as truly explaining not only Torricelli's experiment, but the whole class of phenomena attributed to Nature's horror of a vacuum.

This was one of the first, and assuredly one of the greatest applications of Bacon's "Instances of the Cross,"* and I do not know a more perfect example of the way in which this "help of the understanding" was intended to be used for the interpretation of Nature.

For more than 2000 years men had been traveling along the road pointed out to them by Aristotle; but Torricelli, although he kept to it for some distance, at a certain point constructed a new road, branching off from the other, which he conceived would lead travelers more surely whither they desired to go. But there was no direction post, and when travelers came to the division they knew not whether to still keep to the road of Aristotle, or to turn aside into the branch road of Torricelli, and the generality preferred to continue their journey by the beaten well-known track. It was reserved for Pascal to erect the cross at the juncture of the two roads, to point out the right way to the traveler, and it still keeps its place, and the direction which he graved thereon still remains, and ever will remain, a monument to a glorious intellect.

Some time after hearing of the success of the Puy de Dôme experiment, Pascal requested Perier to observe the different heights at which the column of mercury stood at different times, and during various changes of weather, in the same locality. Perier made daily observation at Clermont, from the beginning of 1649 to the last day of March, 1651. A friend to whom he wrote in Paris made observations from August 1, 1649, to the last day of March, 1651; and MM. des Cartes and Chanut made observations in Stockholm from October 21, 1649, to September 24, 1650.

The following results were obtained:

* Extract from the thirty-sixth aphorism of the Second Book of the "Novum Organum," translated by J. Devey:—"In the fourteenth rank of prerogative instances we will place the Instances of the Cross, borrowing our metaphor from the crosses erected where two roads meet, to point out the different directions. We are wont also to call them decisive and judicial instances, and in some cases instances of the oracle and of command. Their nature is as follows:—When in investigating any nature the understanding is, as it were balanced, and uncertain to which of two or more natures the cause of the required nature should be assigned, on account of the frequent and usual occurrence of several natures, the Instances of the Cross show that the union of one nature with the required nature is firm and indissoluble, whilst that of the other is unsteady and separable; by which means the question is decided, and the first is received as the cause, whilst the other is dismissed and rejected."

At Clermont (about 400 toises above Paris) the greatest height of the column of mercury was 26 inches 11½ lines (February 14, 1651). The least height was 25 inches 8 lines (October 5, 1649).

In Paris the greatest height was 28 inches 7 lines (November 3 and 5, 1649). The least height was 27 inches 3½ lines (October 4, 1649).

In Stockholm the greatest height was 28 inches 7 lines (December 8, 1649). The least height was 26 inches 4¾ lines (May 6, 1650).

Perier noticed the fact that sudden changes in the height of the column often take place; thus, on December 6, 1649, the mercury stood at 27 inches 5 lines, while on December 8 it had risen to 28 inches 7 lines.

It was Pascal's intention to thoroughly explain in a large treatise the principal questions relating to the equilibrium of fluids, the vacuum, and the pressure of the air, but he abandoned this idea, and contented himself with writing two short treatises "On the Equilibrium of Fluids," and "On the Weight of the Mass of Air;" these were found among his papers, and were published in 1663, one year after the death of their illustrious author.

The proofs of the assertions made in the treatise "On the Weight of the Mass of Air," are mainly taken from experiments described in the treatise "On the Equilibrium of Fluids;" the air is considered to act in every way as a liquid, and that which he has found to hold good with water, he considers to be equally true for air. In the first chapter of the treatise "On the Weight of the Mass of Air," Pascal proves that air possesses weight, and that it presses upon all bodies which it surrounds.

In a heap of wool twenty or thirty toises high, the wool at the bottom is more compressed than that in the middle or near the top of the heap, because it is pressed down by a greater weight of wool above it; similarly the air at the bottom of valleys is more compressed than that on the summit of mountains, because it has a greater weight of air above it. If a handful of wool is removed from the bottom of the heap, and placed in the middle or near the top of it it will enlarge itself, because it will have a less weight of wool to support than it had before, so, also, if we take air from a valley and carry it to the summit of a mountain, it will enlarge itself and become in a similar state of density to the air surrounding it, because it will have a less weight of air to support than it had in the valley; this may be proved by partially filling a bladder with air from a deep valley and carrying it to the top of a high mountain—the bladder will become more and more puffed out the higher it is carried.

In the second chapter Pascal proves that the weight of the air produces all those effects hitherto attributed to Nature's horror of a vacuum. This chapter is divided into two sections. In the first Pascal mentions the principal phenomena which had up to that time been attributed to Nature's horror of a vacuum, and in the second he shows how these phenomena are caused by the pressure of the air.

Among the effects attributed to Nature's horror of a vacuum were the following:—

Bellows, all the openings of which are closed, are difficult to open, and the piston of a closed syringe is raised with difficulty.

Two bodies with accurately-ground and polished faces when placed in contact are not readily separated.

When a syringe or pump is placed in water, and the piston raised, water follows it, in order to prevent the formation of a vacuum which would otherwise take place.

When tow is kindled in a plate containing water, and a glass inverted over it, in proportion as the tow is extinguished the water rises into the glass, because the air which was rarefied becomes more and more condensed, and attracts the water with it to fill the place which it quits in contracting, just in the same way that the piston of a syringe draws water after it to fill the space which would otherwise be a vacuum. Cupping-glasses act in a like manner.

A bottle or short tube closed at one end, if filled with water and inverted in a vessel of water, remains filled.

If we open the tap of a tun of wine not a drop of wine escapes until we remove the vent-peg.

The action of an ordinary siphon is due to Nature's horror of a vacuum.

Pascal next adduces proofs that these effects are due to the pressure of the air.

We cannot open closed bellows without exerting a good deal of force, because in opening them we must elevate the whole mass of air resting on them; but if we make a hole in any part of the bellows the air enters, and we have no longer to raise the air above them.

When two well polished bodies are placed in contact, the lower one will remain attached to the other, because the air presses on it below and not above, and as there is no access for the air between the polished surfaces of the bodies, it presses only the upper surface of the upper body, and on the lower surface of the lower body.

Water rises in pumps and syringes placed in a vessel of water when the piston is elevated, because the air presses on all parts of the water in the vessel, except under the piston where the air has no access; as we raise the piston the water follows in order to counterbalance the weight of air which presses on the water outside the pump.

For the same reason water remains suspended in bottles and short tubes closed at one end, which have previously been filled with water and inverted in a vessel of water.

Cupping-glasses attract the flesh, because the air within them is rarefied by fire, and the outward air presses on all parts of the body, except on that part under the cupping-glass, consequently the flesh is caused to swell within the glass.

The air causes water to rise in siphons because it presses on the water of the vessels into which the limbs of the siphon dip, and not on the water in the interior of the siphon.

In the third chapter Pascal proves that the weight of the air is limited, and, therefore, the effects which it produces are limited.

When the sucker of a pump is raised, water follows it to a height

of thirty-one feet, beyond which it refuses to ascend. Mercury refuses to ascend above two feet three inches, and oil above thirty-four feet, the weights are in all cases the same, that is to say, a column of mercury two feet three inches high weighs as much as a column of water 31 feet high, or a column of oil 34 feet high.

When we attempt to open closed bellows, the sides of which are, say a foot diameter, we must, before we can open them, exercise an amount of force capable of raising a column of water 31 feet high and a foot diameter. So also if we have two polished bodies, the diameter of the applied surfaces of which is an inch, we must, in order to separate them, apply a force capable of lifting a column of water an inch diameter and 31 feet high.

In the fourth chapter Pascal asserts that the weight of the mass of air varies according to the vapors which are present in it, and that, therefore, the effects which it produces vary; as an example of this, he mentions the fact that the air will sometimes support a column of water 31½ feet high, and at other times only 30¾ feet high.

In the fifth chapter, the weight of the mass of air is affirmed to be greater in valleys than on the summits of mountains, and the effects produced by it in valleys to be therefore greater than those produced on mountains.

At a height of 500 or 600 toises, a suspended column of water is 4 feet shorter than at the level of the sea. Moreover, two polished bodies which have been applied to each other are separated with greater ease on the summit of a mountain than at its base.

The mercurial column falls about 1 line for every 10 toises of height.

In the sixth chapter Pascal asserts that as the effects produced by the weight of air increase or diminsh, as it increases or diminishes they will entirely disappear above the air, or in a place where there is no air.

As we find the weight of a column of mercury becomes less and less the higher we ascend, we have a right to conclude that the mercury would entirely fall if we could go above the air, or in a place where there is no air; but as this is impossible, we may try the same experiment by completely removing the air from the open orifice of the mercury tube; in order to do this, Pascal connected the open end of one barometer tube with the upper part of a second tube, filled them both with mercury, and placed the open orifice of the lower tube in mercury, the mercury in the upper tube immediately left it, because no air was present to counterbalance its weight.

According to a calculation, which is given in full in chapter 9 of this treatise, Pascal estimates the weight of the entire mass of air which surrounds the earth at 8,283,889,440,000,000,000 livres.*

Among those papers which were found after the death of Pascal there was one containing a number of tables, giving the results of experiments made to determine the weight necessary to separate two

* A livre = 1 lb. 1 oz. 10¼ dr. Avoirdupois.

polished surfaces from each other, the diameter of the applied faces being respectively 1 foot, 6 inches, 1 inch, and 6 lines.

The experiments were made in Paris, about 31·5 toises above the level of the sea. At Clermont, about 400 toises above Paris; at Lafon, 600 toises above Paris; and on the summit of the Puy de Dôme, 900 toises above Paris. The following table represents the weights in livres necessary to separate two polished bodies 1 foot in diameter:—

	Greatest weight necessary.	Medium.	Least.	Difference.
In Paris, . .	1808	1761	1714	94
At Clermont, .	1675	1628	1581	94
At Lafon, . .	1579	1532	1485	94
Summit of Puy, .	1483	1436	1389	94

(To be Continued.)

Refrigerating and Ice-Making Machines.

From the Practical Mechanic's Journal, November, 1863.

(Continued from page 114.)

Passage from the Refrigerator to the Boiler.—The power of the apparatus is proportional to the latent heat of the volatile liquid and to the weight of vapor which forms per hour in the refrigerator. This weight of vapor is only dependent on two things; on the form of the refrigerator, and on the difference which exists between the general elastic force existing in its interior, and the maximum elastic force due to this vapor, at the temperature of the liquid which wets the interior surfaces. In fact, if the interior were saturated with vapor no new vapor could be formed and no cold produced. If, on the contrary, the interior were maintained without vapor, that is to say, in the state of a perfect vacuum, the weight of vapor, formed per hour, would be at its maximum, and the production of cold would also attain its maximum. It is necessary then to exhaust this vapor, which is no sooner formed than it becomes an obstacle, the space which it encumbers must be freed, so as to restore it incessantly to a state of perfect vacuum, or at least as nearly so as possible. There are different means for this, but incontestably the most advantageous is that which can be employed here, that is, to exhibit a body which can rapidly condense it by a dissolving affinity, and which can give it off again with the same rapidity by a sufficient increase of temperature. The boiler is so arranged that the ammoniacal solution, at its lower part, is very weak; a pipe furnished with a stopcock is placed there to withdraw a certain volume of it, which is fixed by the degree to which the cock is opened; this tube, made of iron, and 20 or 30 metres long, is coiled twice at different distances into two helical worms, which are surrounded with cooling liquids. Thus, the liquid contained in the tube, starting from the boiler at 130° and cooled to about 20° to 25° C., reaches the top of the absorbing reservoir to fall as rain into its interior. It is this continual rain of impoverished liquid which becomes the power capable of incessantly maintaining and renewing the vacuum in the interior of the refrigerator. For this purpose a large pipe, some metres long, connects the top of the refrigerator with the top of the absorbing reservoir. As soon as the cock which regulates

this communication is opened, the ammoniacal vapors of the refrigerator flow into the midst of the rain of weak liquor, are there condensed by absorption, and reconstitute a rich liquor which falls to the bottom of the reservoir. The heat which is here disengaged is removed by the coils of the worm pipe through which cold water flows; it only remains to take up this rich liquor, and re-introduce it into the boiler, so as to make up for the loss of ammonia which takes place every instant, or rather to restore all which has left it, and thus to close this long circulation, in which change of form and state takes place, but no loss or gain of matter.

It is a lift and force pump of a very peculiar construction, and well adapted to the effect it is designed to produce, which is arranged to accomplish this last part of the circulation. It draws from the bottom of the absorbing reservoir the liquid which has been enriched as fast as it forms, and then causes it to enter a special vessel designed to receive it; it then forces it through a long tube where it is re-heated, to arrive at last at the top of the cascade of which we have previously spoken, and which constitutes the top part of the boiler. This liquid, although re-heated in its course, is far from being at 130° C.: its presence then produces some condensation, the effect of which is no doubt favorable to the rectification of the hydrated vapors of ammonia which exist at this point.

We cannot close this summary description of the apparatus without remarking that we have been obliged to pass over a great number of details, adjustments, and ingenious arrangements, which perhaps prove even more than the general design, the genius, and resources of the inventor. Let us now endeavor to explain to what the economic power of the apparatus extends, and its limits.

This discussion rests on but a small number of data, namely:—On the latent heat of vapor of the ammoniacal liquor, and of the more or less dilute solutions of ammonia. Davy has already drawn up a table of the quantity of ammonia contained in solutions of different densities; this table, which is based on only two experiments, is re-produced in all the chemical books; it is desirable that it should be again taken in hand and extended to different temperatures. In applying it to the practical observations of M. Carré, we are led to believe that in the actual state of things, one kilogramme of the weak solution which enters the absorbing reservoir cold, can there take up 50 grammes of ammonia to become the rich ammonia which is re-introduced into the boiler.

Our fellow-worker, M. Regnault, has been good enough to give to the Commission a proof, extracted from his great work on vapors, which is now in the press. We there find a complete table of the tensions of ammonia solution between the temperatures of 40° and 100°, for the lower temperatures, which are of the most importance here. These tensions are—

Temperatures, . . .	20°	30°	40° C.
Tensions in atmosphere, . .	1·84	1·16	0·70

To apply these numbers to the slightly dilute solution of the refrige-

rator, we must take account of the diminution which they have to undergo. Estimating this at one-fourth, we get the following result—

Temperature of refrigerator, . .	20°	30°	40° C.
Tension in atmospheres, . . .	1·4	0·9	0·5

which approaches closely to the practical observations of M. Carré.

Lastly, according to the researches of MM. Favre & Silbermann, (Annales de Chimie, t. xxxvii., 1853,) we may value at about 500 units, the latent heat of ammoniacal gas absorbed by a mass of water large enough to form a dilute solution. We admit it as probable, that this number may be applicable to ammonia containing very little water.

It results from these data, that to construct an apparatus whose power shall be, say, 100,000 units of heat an hour, it will be necessary to vaporize 200 kilogrammes of ammonia in the refrigerator, and thus, at the same time 200 kilogrammes must be condensed in the liquifier, and absorbed or condensed a second time in the absorbing reservoir. The 100,00 units of heat, or very nearly, re-appear then in these two members of the apparatus, where they must be taken up and carried off by the cooling water.

Admitting that the temperature of this cooling water only rises 10° in this operation, we see that the expenditure of it will be 20,000 kilogrammes, or 20 cubic metres an hour; that is, 10 cubic metres to cool the liquifier, and 10 cubic metres to cool the absorbing reservoir.

We do not speak of the necessary consumption of fuel in the boiler. In effective result, this should also be equivalent to 100,000 heat units an hour, but there are unavoidable losses, which are very variable.

In a word, the four changes of state, although operating under different conditions, should be accompanied by the same, or nearly the same, phenomena, as far as regards the quantities of heat concerned.

The boiler and the refrigerator acting by evaporation, take up the same quantity of heat, one from the fire, and the other from the liquid which it cools. The liquifier and the absorbing reservoir acting by liquefaction, should disengage the same quantity of heat, which they must get rid of by continually changed cooling liquids. The mechanical work of the lift and force-pump should also be approximately valued.

Since 200 kilogrammes of vapor are produced an hour in the refrigerator, 4000 kilogrammes of weak solution are required to absorb them, for each kilogramme only absorbs 50 grammes, or one-twentieth of its weight. The result then will be 4200 kilogrammes of strong solution. The work consumed in re-introducing this into the boiler, the necessary pressure being estimated at 10 atmospheres, will consequently be 420,000 kilogrammetres, or about two horse power, to which must be added about one-tenth for the work consumed in suction; but in this the considerable loss of power in working the pump by the disengagement of elastic fluids is neglected.

As to the degree or intensity of cold which the apparatus can produce, it depends almost exclusively on the phenomena which take place in the absorbing reservoir, because there, in fact, exists the determining cause of the rapid formation of vapor in the refrigerator. If, on the one hand, the liquid which gives off this vapor were pure ammonia,

free from water; and if, on the other, the weak liquor which comes from the boiler into the absorbing reservoir were pure water, free from ammoniacial gas, there can be no doubt that the refrigerator would easily fall to 50° or 60° C., below zero.

But in reality, the liquid in the refrigerator contains a certain proportion of water, and the weak liquor which enters the absorbing reservoir contains a very perceptible quantity of ammonia. These two causes join in relaxing the absorption of the vapor, and consequently in preventing the degree of cold descending so low in the thermometric scale. Hence there is room for study in making the boiler yield a liquor still poorer in ammonia, and the liquifier a liquid more completely deprived of water.

This ultimate limit to the economic power of the apparatus always depends on still another circumstance: it varies necessarily with the temperature of the air, and consequently with seasons and climates. Let us suppose, for instance, that the refrigerator *works in space*, that is to say, without making ice or cooling a liquid; in a word, without producing *useful effect*, it will arrive nevertheless at a certain limit of cold, which will be its extreme limit, say 50° below zero. Suppose that in this experiment the air has a temperature of 10° C., this gives it an excess of 60° over the refrigerator. Once arrived at this limit, after working for a longer or shorter time, during which the refrigerator might be observed gaining progressively in cold, very quickly at first, and more gently afterwards, it may be asked, How is it possible to maintain this state of things? May we draw the fire from the boiler, stop the pump, and in fact set the apparatus at rest, without the refrigerator becoming warm? Assuredly not. On the contrary, it is indispensable to continue working at full speed. Its entire power is then lost, in the sense of its not being utilized; but it is not lost in reality, for it is wholly employed in maintaining the refrigerator in equilibrium against the inroads of heat from without. We can stop the motion of a machine, but it is not given to us to stop the motion of heat. Whatever precautions are taken to protect the refrigerator, heat always penetrates to it, although the rate at which it does so may be more or less reduced. The number of heat units which thus reaches the refrigerator in a given time, all other things being the same, is nearly proportional to the extent of surface which it presents, more or less directly, to the air, and to the excess of the temperature of the air over that of the refrigerator.

Consequently, if the same apparatus is submitted to a second test in space, with the air at 30° C., instead of 10°, it can never, work as it will, cause the refrigerator to descend to 50° below zero, but will only make it fall to 30° below zero, because the excess of the temperature of the air over the refrigerator will be again 60°, as it was in the first test.

The preceding considerations also permit the inference, that a refrigerator designed for making ice will be much more favorably circumstanced for descending to very low temperatures than one designed for merely *cooling* liquids, because in general the latter will offer much greater surfaces than the former to the access of heat from without.

Such are the principal causes which vary the extreme limit of cold to which the refrigerator can fall, and which vary the economical power of the apparatus in the same sense.

It results from this, that the weight of vapor which forms in an hour in the refrigerator of a given apparatus should be considered as nearly a constant quantity, but composed of two parts, which do different duties. The first produces useful effect; the power of the second is lost. This latter, without ever becoming nil, remains very small when the refrigerator, to perform the effect required is at a temperature considerably removed from its extreme limit; but it increases rather quickly, and always with a detrimental effect, whenever the refrigerator has to work at a very low temperature; and at last it absorbs the whole or nearly the whole, of the useful effect, if the refrigerator has to work at a temperature very near its extreme limit.

These variable losses depend on co-efficients, to which experience will soon give values sufficiently accurate to estimate the useful effect, according to the nature of the work to be done, and the knowledge of the exterior temperatures to which the apparatus must be exposed.

M. Carré has carefully studied all the theoretical principles which should guide him in the construction of his apparatus; he has aided himself in this way by studying with great sagacity all the new questions which bear on the original problem, and he has ultimately arrived at solutions which have the merit of being at the same time highly ingenious and very practical.

"The Commission is of opinion, that the apparatus of M. Carré is capable of rendering real service, and proposes to the Academy to admit the description of it into the 'Recueil des Savants étrangers.'"

The conclusions of this Report were adopted by the Academy.

Poisoning of Water by Lead Pipes.

From the London Mechanic's Magazine, May, 1864.

Professor H. Dussance, of New York, has recently made a series of experiments on the action of several different kinds of water on lead, under various conditions. The lead was subjected to the action for twenty days, and the experimenter draws the following conclusions:— 1. That distilled water has no action whatever on lead by three days of contact; after that time this dissolving action begins. 2. That the lead is dissolved by distilled water in proportion increasing every day: the distilled water exposed to the open air dissolves more of this metal than distilled water in close vessels, or than distilled water deprived of air and gas. 3. That creek water, containing small proportions of lime, has no action on lead. 4. That distilled water, containing 1-3500th of a salt in solution, prevents the dissolving action of the water on lead. 5. That water dissolves lead till the saturating power of the acid is exhausted. 6. That, in ferruginous water, all of the iron is precipitated by lead; then lead pipe must not be used to convey mineral waters. This fact has never been noticed before. To render these facts more interesting, another series of experiments must be made to ascertain

the quantities of lead dissolved daily in the water, and what compound it forms, and to see if the action will be the same in the lead pipes.

Among all the dangers arising from carelessness, the public may not be aware, that there is great danger in the careless use of even hydrant water. That water, impregnated with any preparation of lead, cannot be safely used for culinary purposes, even in cases where the lead is present in too small a proportion to produce any immediate injury, is well known. Lead, says Professor Aikin, of New York, like some other poisonous agents, when taken in minute successive doses, will remain in the system, apparently inactive, until a certain amount accumulates; then, suddenly, a series of alarming symptoms will supervene, often resulting in death. An occurrence of this kind was recently brought to my notice by one of my professional friends, who was called upon to prescribe for several members of the same family, all apparently suffering from the peculiar action of lead. In one of the cases the result was fatal—the others, after much suffering, slowly recovering. A portion of the water supposed to have been instrumental in producing the difficulty was brought to me for examination, and carefully analyzed, and found to contain lead in the proportion of 0·028 gr. to the gallon of water. This very minute quantity, very little exceeding the 0·001 gr. to an ordinary tumbler full of water, taken at intervals, somewhat regularly, seems to have accumulated until the poisonous action of the metal was developed. The whole difficulty seems to have arisen from the occasional use of water taken from a hydrant stop that was not in regular and frequent use; the water in a portion of the lead pipe being thus allowed long continued contact with the metal in the immediate vicinity of the stop. The simple precaution hereby indicated, and one that cannot be neglected with safety, is to allow the first discharge of water from any hydrant stop to run to waste, and not to collect any for culinary purposes until we are sure that all the previously stagnant water in the lead pipe has wholly escaped. And it would be better, in general, to resort to the yard hydrant for water required for drinking or preparing food—the lesser length of lead pipe there in use diminishing the danger. But even there the water first discharged in the morning should be thrown away.

Electro-Magnet Induction Machines for Telegraphy.

From the London Chemical News, No. 223.

A new induction machine is now to be seen in operation which produces a constant stream of electricity, and can be made to produce it of any tension or quantity that may be required. These are valuable properties, which doubtless will be appreciated by all those who are obliged to use galvanic batteries of high tension. Many attempts have been made to use induced electricity for telegraphy, but as these attempts have been generally made with machines similar to Rhumkorf's induction coil, they have failed, for the following reasons:—The electricity is in impulses, and alternately in reverse directions. It is in too small quantity, and too great tension; so much so, that it is said that

Mr. Whitehouse's five-feet coil destroyed the Atlantic cable; but the authorities are not agreed on the point. However, it is certain that such machines are not suitable for telegraphy generally, even if one impulse be thrown out, so that all the impulses which are taken may be in the same direction; for there is then no approach to a battery current. This has been done, but in all cases the most suitable impulse has been rejected, namely, the slow one, or making impulse, and the breaking impulse taken, which is of greater tension but shorter duration; of course, the absolute quantity of force in each is the same, and it will be clear that the former, approaching more nearly to a current, would be most suitable. In some cases the two reverse impulses are used; but these can only be used with one telegraphing arrangement, and that a slow one.

What has been wanted, and which is now obtained, is as near an approach to a battery current as may be, and of any tension required, without multiplying the number of battery cells used. Any machine to be of real use must not only possess these properties, but must be perfectly self-acting. The machine here spoken of possesses all these properties. Its current is perfectly continuous for all practical purposes, for no intermittance can be detected. It holds the needle perfectly steady, rings an electro-magnet bell, produces electrolysis, and comports itself in every other way like a battery current, even producing no spark unless the poles be brought into absolute contact. The electricity which is used to excite the electro-magnets in the coils is at the same time made use of as magnetism in the electro-magnets to drive the battery break and commutator; so that the motive power, which makes this machine self-acting, is obtained without cost, because to produce induction the magnets in the coils must necessarily be excited whether the magnetism so induced be otherwise used or not.

Two series of induction coils are used in this machine, which are so arranged that one series is being magnetized nearly at the same time that the magnetism is subsiding in the other, so that the two consequent induced impulses as it were overlap each other; and though these induced impulses are in opposite directions, the spools are so arranged that in the general induction circuit they flow in the same direction, thus making a compound impulse of longer duration composed of the two opposite inductions, and so blending the more intense with the less, the result being a sort of undulation of force. These compound impulses flow in opposite directions, and to make them a continuous flow they are taken up by a commutator which reverses its contacts as they reverse, and thus turns them all in one direction, producing a slightly undulating but continuous flow. This seems to be the form of motion that all known forces take. The machine appears to be made for quantity, the inner coils being of No. 12 wire, and the outer of No. 18. Some electricians who have examined it, have expressed a fear that it might injure the insulation of gutta perchaed wire, as this fault has been attributed to ordinary induction coils. Of course, this objection does not apply to this machine any more than to an ordinary battery; for to whatever purpose it may be applied its tension and quantity will

be adjusted to the tension and quantity of the number of battery cells ordinarily required to perform that work; nevertheless, the machine is being subjected to continuous tests to put the thing beyond doubt. A gutta-percha wire is punctured and then immersed in water, and the leakage tested, then the full current sent through it for hours, but as yet no perceptible variation has been noticed.

Tubular Boilers.

From the London Mechanics' Magazine, July, 1864.

There is not a question connected with the steam engine or the employment of steam power which has not at one time or another been made the subject matter for bitter discussion. The value of the heating surface of tube flues, in particular, stands forth prominently, as one of the most fertile soils for the production of this kind of argument that has yet been cultivated. We question if two men of science who have given any thought to the matter, can be found at this moment who will agree even on the conditions under which the tubular boiler can be worked to the most advantage. As to its comparative abstract value, it seems all but hopeless to expect an unanimity of opinion. Very many and very elaborate experiments have been conducted from time to time, but, regarded in the proper light, these experiments impart very little information. As a rule, they have been two-fold in their object. In the first place, they have been instituted to determine the value of flue surface as compared with fire-box surface in the same boiler; and, secondly, they have now and again been employed to determine the value of the tubular as compared with the Cornish or the externally fired boiler. As to the first, it is somewhat remarkable that there are no recorded instances of experiments conducted under other than abnormal conditions, that is to say, the generator has never been employed as generators of the class would be in actual work. As to the second, we are at a loss to find an instance wherein precisely the same coals, the same stack and draft, and the same water supply were employed in each case, and it is yet more difficult still, to find an instance wherein all parties were not previously impressed with the superiority of one or other class of boiler over that with which it has been compared. In spite of the utmost conscientiousness on the part of the experimentalist, such a bias is certain to produce results on which it is difficult to place implicit reliance.

The truth of the matter is this: apart from its economical value, the tubular boiler possesses certain constructive advantages which have brought it into high favor. No other practical generator—by which we mean one moderately cheap and not likely soon to get out of order—will bear a moment's comparison with the tubular in its powers of making steam, weight for weight, and size for size. Lighter boilers may be found of equal power, but they are inevitably larger; smaller boilers may be found, but they are sure to be heavier. And, beside all this, the shape of the tubular boiler of the best type is, above all others,

that best adapted for a locomotive carriage supported on more than one pair of wheels, and intended to travel at high speeds. This fact in itself is enough to render its popularity for railway purposes a thing which needs no further explanation. Again, for marine purposes, nothing more convenient than the return flue system exists, and without tubes it is not easy to carry out this principle of construction with any success.

Every tube flue has three purposes to fulfil at least. Occasionally it may have to perform a fourth, to which we shall refer further on. The first is to permit the passage of its own proportions of the products of combustion from the furnace to the chimney. The second, to transmit through its walls the greatest allowable quantity of the heat of these products to the water; and the third purpose is, under the given conditions, to fulfil its duties better and more economically than any other device. We shall consider the last first, because it is impossible to put a flue to work until it is made, and on its material a great deal of its value will ultimately depend. The first tubes used appear to have been of copper. The tubular boiler proper was employed in France before it came into favor, even in the idea, here. The first instance of its actual use on record appears to have occurred in the case of two of Stevenson's locomotive engines, fitted in 1828 with the tubular boilers by M. Marc Seguin, engineer of the St. Etienne railway. Stephenson used copper flues in the first instance in the "Rocket." Shortly afterwards their use became habitual in America, especially for wood burning engines, and they are used in that country still, although rarely. Now copper appears at first sight to be a very excellent material for flue-tubes. It can be easily drawn into pipes; hard solder may be employed to make up the joints; its ductility is such that the tube expands under the action of the ferrules in the tube plates, and is, therefore, very easily fitted into place without any necessity for nice workmanship; and, above all, the metal is one of the best conductors of heat known, the relative thermal resistance of iron and copper being as 96 to 40. No wonder, therefore, that the use of copper flues was habitual for some time after the railway system, as applied to the conveyance of passengers, came into operation. But with the good qualities that we have named, all that is good about copper tubes begins and ends, and very little practice proved that the particles of flying coke cut them up so rapidly that they required continual replacement at a very heavy expense. It is well to bear in mind, therefore, that copper, theoretically the best, is practically the worst material ever used to make tubular flues. After the failure of copper, brass was introduced, in 1833, as a material for locomotive flues, on the Liverpool and Manchester railway; as far as we can learn, at the suggestion of Mr. Dixon, then resident engineer of that line. The copper flues lasted on an average three months; the brass two years at least. Brass possesses many of the constructive advantages of copper, while its superior hardness imparts to it the quality of permanence so desirable. Hundreds of tons of brass tubes are used, therefore yearly, for locomotive purposes. Iron tubes, though rather more difficult to set, apparently remain tight

longer, and are as a rule more durable than brass; they are a great deal cheaper as well, and in consequence they are extensively used; indeed, it is questionable if they may not one day turn brass out of the market. Thus we see that either iron or brass are better calculated to answer the third great purpose of a flue than any other material. Steel may yet play an important part, of course; but, in the absence of extended experience in the use of steel flues, it would be premature to speak positively as to their value.

The second great purpose to be fulfilled by a flue is the transmission of heat to the water with which, in common parlance, it is in contact. It may be said that this is really the first object; but such is not the fact, because it is impossible that the tube should have any heat to transmit, unless the products of combustion can pass freely through it in the first instance; and the neglect of the principle involved in this statement has been the cause, time and again, of great disappointment. We can state, without hesitation, that the materials of which a flue is composed does not practically exert the smallest influence on its evaporative powers. It is true that both brass and copper are very much better conductors of caloric than iron. Yet iron flues are quite as efficient and as economical, in all that concerns the use of fuel, as either the one or the other of the dearer metals we have named. Why this should be so has excited a great deal of unnecessary and useless speculation. In point of fact, the water within a tubular boiler is never in contact with the metal of the tube. The same holds true of the heated gases. A coating of deposit of greater or lesser thickness collects after the first few days of the active life of a new boiler on the exterior of the flues, and a coating of soot forms within. The conducting powers of these two substances, really measure the calorificant value of the tube. When experiments are conducted in the laboratory on this subject, the metallic surfaces are all kept clean, perhaps bright. Could the flues of a locomotive be kept bright also, there is no doubt that the results obtained in the laboratory would be verified in practice. Thus we find that, during the first few days a brass-flued boiler always makes more steam than one the flues of which are of iron. With the appearance of a little deposit this superiority becomes evanescent. Deposit is one of the most perfect non-conductors of heat in existence, and so greatly does it obstruct the passage of caloric, that the difference between the relative values of copper and iron is as nothing, when compared with the relative values of iron, and copper, and deposit, as conductors. While each tube is enveloped in a second strongly adherent to it, and formed of lime and clay, it is a matter of very little consequence of what the tube itself is made. Even with surface condensation it is impossible, practically impossible, to keep the inside of a boiler clean. If there is not sulphate of lime, there is certain to be oxide of iron arising from the decay and corrosion of the plates. No laboratory experiments have yet been undertaken to determine the relative value of encrusted flues of different metals; simply, we presume, because it was evident beforehand that no appreciable difference would be found to exist. To put the fact in the simplest light, it is

the deposit which directly imparts heat to the water with which it, and not the tube, is in contact. All that the metal of the flue can possibly do is involved in the transmission of caloric to the deposit in the first instance.

We have stated that a great number of experiments have been undertaken to determine the relative value of tube and fire-box surface. The results of these have been presented to the world as conclusive. Yet they are, one and all, untrustworthy, and to a certain extent fallacious. In 1830 Stephenson took the top off a locomotive boiler; a tube-plate separated the water in the fire-box shell from that in the barrel of the boiler. A fire was lit in the box, and the results obtained went to show that one foot of fire-box was equivalent to three of tube surface. In 1840 Mr. Dewrance performed the same experiment with a difference. He divided a small tubular boiler into seven distinct compartments, the first being fire-box, the second six inches of tube, and the remaining five, a foot each of tube, From this experiment it was deduced that the first six inches of tube were equal, square foot for square foot, to the fire-box surface; the second compartment was about one-third as effective; and for the rest, Mr. Dewrance stated that the evaporation was so small as to be practically useless. Mr. Wye Williams, not many years ago, fitted up a tube five feet long, and three inches in diameter, in his laboratory. This tube passed through a vessel divided into five compartments, each a foot long; the heat was applied by means of a ring of gas jets fitted in one end of the tube, turned down at right angles. The temperature of the waste heat was said to have been 800°, yet the boiling point was never once reached in the compartment furthest from the source of heat. Not one of these experiments possesses much in common with the conditions under which a tube works in a locomotive boiler, simply because the influence of the blast in the chimney has invariably been neglected. The caloric to be derived by conduction from heated air is very trifling, because hot air parts with caloric with great reluctance. With true flame the case is altogether different; the most intense heats known in the arts are the result of flame. The blow-pipe, the lime-light, nay, even the combustion of gas in a common paraffin lamp, are illustrations of the truth of this proposition. No one ever attempted with success to fuze platinum, or decompose the diamond, with a jet of heated air; flame has alone proved equal to the task. It is certain that water takes up heat very slowly from a gas unless in actual contact with it, and the absolute quantity of caloric which will penetrate a metal plate apparently varies nearly as the cube of the intensity of the sensible heat. Why this should be so we know not; and the entire problem has been very much overlooked by experimentalists. The relative calorific value, then, of flame is transcendent as compared with hot air, and this indisputable fact once admitted, there is no longer any difficulty in seeing wherein, Stephenson's, Dewrance's, and Wye Williams' experiments were defective. The belief obtained then, as it does now, that flame would not enter a small tube. The inventor of a safety-lamp depending on this principle for its success was not likely to dispute the question; especially with the

example of the Davy-lamp—a far more perfect invention—before his eyes. We are told nothing of a forced draft in Mr. Dewrance's experiments, and from the nature of those conducted by Mr. Williams it is simply impossible that the flame could enter the tube he used. In the case of a locomotive burning coal, or even a gaseous coke, there is no doubt whatever that, with a powerful blast, flame does traverse the tubes in whole or in part of their length; the distance depending on their diameter and the force of the draft. As a datum from which deductions may be drawn, we may state that flame—true bright flame—will traverse a tube 2¾ ins. diameter, 6 feet long, at each stroke of the piston, and we feel very little doubt that in coal-burning engines the tube surface invariably possesses a higher economic value in proportion to the fire-box than when coke is burned. We said there was a fourth purpose which the tube flue might fulfil; and it is involved in permitting the combustion of gas and the conequent production of flame, to go on within it. Certainly, it is somewhat remarkable that, while Mr. Williams could not make water boil by means of air heated to 800°, steam of half the same temperature will, if conveyed through a worm, cause water to boil freely in an open vessel. It is known that locomotives with long flues of small diameter are unable to make steam without a tremendous blast. The cause has been sought in the friction of the air against the sides of the pipes. Doubtless, there is a certain amount of truth in this, but not all the truth. Flame will not enter a very small tube very far, except under strong compulsion, and there is little doubt that in such cases the value of the tube surface falls off so much in consequence of the absence of flame within them, that the evaporative powers of the boilers suffer considerably. A great number of small tubes are never so efficient individually as a smaller number of large ones. As to the relative durability, absence of priming, and efficacy, the over-flued boiler will bear no comparison with its rival.

The first great purpose of a flue is the conveyance of the products of combustion to the smoke box. The concluding sentences of the last paragraph state nearly all that can be said on this branch of the subject. The proportion which the calorimeter of a boiler—in other words, the area of its flues or tubes—should bear to the area of grate bar, is a question of considerable importance. In the case of the locomotive, it is intimately connected with the position of the blast pipe, its area, and the mode in which it is fitted. In the marine boiler the cubic contents of the furnace, the power of the draft, the quantity of the coals, &c., all exercise a considerable influence. We purpose returning to this branch of the subject at another time.

Theoretical and Practical Researches on the Formation of Positive Photographic Proofs. By MM. DAVANNE and GIRARD.

From the London Chemical News, No. 232.

Photographic images, especially those known as positive proofs, are the result of remarkable transformations, which might, unless properly

studied, be considered anomalies. In each new operation the color, clearness, intensity, and solidity of the result may be very considerably varied, without the cause of the variations being apparent. These hitherto unexplained transformations may, nevertheless, be classed with ordinary chemical reactions. This is what we have endeavored to prove by a series of researches extending over six years.

To produce a positive proof the photographer takes a sheet of paper, covered with a sizing of albumen, gelatine, or starch, impregnates it with a soluble chloride, and then submits it to the sensitive action of a solution of silver. The image is then ready for insolation; placed under a negative plate it re-produces inversely the most delicate details. At this moment it is very brilliant, but will fade if not fixed by re-agents, which dissolve on the unacted salts; and its coloration would be confined to the red tints given by contact with the fixers were it not finally submitted to the action of the coloring liquids called toning agents.

These successive operations require all the attention of the chemist; we have followed them step by step, and each has revealed to us some new facts, of which we will give a succinct *résumé*.

On Paper.—It is a fact well known to photographers that proofs prepared under the same conditions, but on papers of different origin, produce tones varying extremely. The cause of these variations arise from the influence exercised by the sizing of the photographic papers. A proof on unsized paper when taken from the fixing bath is always grey and flat; while on gelatinized, albuminized, or starched paper, it always has a red brilliant tone, increasing in vigor with the abundance of the sizing. In this case a combination takes place between the sizing and the silver compounds, and this combination, a true lake color, manifests its influence towards the finishing of the proof. The fact may easily be directly demonstrated: a mixture of chloride and nitrate of silver long exposed to the light, then heated with hyposulphite of soda, leaves as residue a grey metallic powder; while the same mixture, with the addition of gelatine, albumen, or starch, gives, under the same circumstances, a matter which gradually dries under the form of a brilliant red varnish, and which is found, on analysis, to contain carbon, hydrogen, and nitrogen. This argento-organic lake plays a considerable part in obtaining a photographic image; we shall have fresh occasion to appreciate its importance when investigating the causes of the alteration of the proofs.

The Salting.—The first operation which the paper undergoes is the imbibition of a soluble chloride; for this purpose chloride of sodium is usually employed, but certain authors recommend various other metallic chlorides, attributing special qualities to them. We have shown that the differences in action of these chlorides are more apparent than real; they are always to be ascribed to the varying excess of acid with which the salts are impregnated. With any chloride very various coloring may be obtained; mixed with an excess of acid or alkali, this chloride will always give a redder tinge than if employed in a neutral state; the natural explanation of this result is to be found in the nor-

mal action of acids and alkalies on the organic matters employed for sizing.

On Rendering Sensitive.—Chlorinated and dried, the paper is then placed on a bath of nitrate of silver; three things thus result from this and the sensitive surface on coming from this bath is formed of chloride of silver, of a combination of gelatine, albumen, or starch, with nitrate of silver; and, lastly, of an excess of free nitrate of silver. The presence of these three substances is indispensable to the production of a good proof; chloride of silver alone gives but a dull and superficial image, but it produces the image rapidly; nitrate in excess gives the necessary depth of this image, and the argentico-organic lake gives it the characteristic red color. The sensitivizing bath may vary in richness, and we have carefully studied the effect of these variations; it may be neutral acid or alkaline; in the two latter cases the effect is the same as if the acid or alkali had been added to the chloride bath.

On Insolation.—To determine the effect of light on the sensitive surface, the composition of which we have given, is certainly, from a theoretical point of view, the most important part of our researches. It is universally admitted that chloride of silver exposed to the solar rays undergoes decomposition and disengages part of its chlorine; but the question is really much more complex than at first appears, and it is necessary to ascertain not only what becomes of the chloride, but also what becomes of the argentico-organic combination of free nitre.

First as to the chloride. It has long been thought that light reduced this body to the state of sub-chloride Ag_2Cl. We have shown that such is not the case, and we stated that the decomposed chloride separates entirely as chlorine and metallic silver. We have established this, the principal point, by first proving that the product of the action of light on chloride of silver is soluable in hot nitric acid, while the essential property of sub-chloride Ag_2Cl is its insolubility in this re-agent, and that this product, freed by hyposulphite of soda from the unreduced chloride of silver, contains no trace of chlorine.

It has certainly been objected to this last proof that hyposulphite of soda used as a fixing agent might have decomposed the sub-chloride Ag_2Cl into the chloride $AgCl$, which would there dissolve into metallic silver; but the only good experiment cited in support of this hypothesis is the change in the color of the insolated proof when placed in contact with the fixing agent. Now, we have ascertained that this change of color is due to quite another cause, to a hydratation of the argentico-organic lake, and that this result is to be obtained by simply exposing the proof to the vapors of boiling water. Chloride of silver, then, is transformed by the action of light into chlorine and metallic silver.

It is the disengagement of chlorine from this decomposition which makes nitrate of silver play so important a part in positive photography. Thus, as we have already observed, a proof obtained with chloride of silver only is always tame and effective; in presence of excess of nitrate, on the contrary, it acquires great brilliancy. This result

is easily explained; in fact, the action of light on a uniform surface of chloride of silver is soon limited by the opaque layer produced by the superficial reduction of the argentic compound; but if this compound is mixed with free nitrate of silver then, beside the portions reduced, and under the influence of the chlorine, they disengage, there are formed new quantities of chloride, which may then be attacked by light, because before this, in the state of nitrate, these portions occupied a particular place, and the reduced silver did not hitherto recover them; so that instead of a dull image several excessive layers are formed, which give to the design the required depth.

At the same time that the chloride of silver is reduced to a metallic state the argentico-organic combination is also reduced, forming a sort of insoluble lake, which, hydrating by contact with alkaline fixers, communicates a very pronounced red coloring to the proof.

The applications of these fixers is the second phase in the manipulations required in positive photography.—*Comptes-Rendus*, lviii., 634.

Description of a New Barometer.

From the Journal of the Society of Arts, No. 593.

This barometer consists of a column of mercury placed in a glass tube hermetically sealed at the top, and perfectly open at the bottom. The lower half of the tube is of larger bore than the upper. If a column of mercury, of exactly the length which the atmosphere, at the given time, is capable of supporting, were placed in a tube of glass, hermetically sealed at the top, of equal bore from end to end, the mercury would be held in suspense; but immediately the pressure of the atmosphere increased, the mercury would rise towards the top of the tube, and remain there till, on the pressure decreasing, it would fall towards the bottom, and the portion which the atmosphere was unable to support would drop out. If, however, the lower half of the tube be made a little larger in the bore than the upper, then, when the column falls, the upper portion passes out of the smaller part of the tube into the larger, and, owing to the greater capacity of the latter, the lower end of the column of mercury does not sink to the same extent as the upper end, and the column, as a matter of course, becomes shorter. This falling will continue until the column is reduced to that length which the atmosphere is capable of supporting, and the scale attached thus registers this fall, or what is ordinarily termed the height of the barometer. From the above description it will be evident that, by merely varying the proportions in the sizes of the two parts of the tube a scale of any length can be obtained. For example, if the tubes are very nearly the same size in bore, the column has to pass through a great distance before the necessary compensation takes place, and a very long scale may thus be obtained, say ten inches for every one inch rise and fall in the ordinary barometer. But if the lower tube is made much longer than the upper, the mercury passing into it quickly compensates, and a small scale, say from two to three inches for every inch, is obtained. To ascertain how many inches this rise

and fall for an ordinary inch of the barometer would be, the inventor, Mr. Hicks, of Hatton-garden, attaches his barometer with a standard barometer to an air-pump receiver, and thus ascertains the scale for every inch, from 31 to 27 inches. On the same principle, as regards the tube, the inventor has constructed an absolute standard barometer graduating the scale from the centre, and reading it off at each end of the column with two verniers, to the one-thousandth of an inch. To ascertain the height of the barometer graduated in this way, he takes a reading of the upper surface of the column of mercury with one vernier, then of the lower surface in the same way with the other vernier, and by adding the two readings together, he gets the exact length of the column of mercury supported in the air, which is the true height of the barometer at the time. Gay-Lussac's pipette is introduced into the centre of the tube to prevent the possibility of any air passing up into the top. Such a barometer is convenient for measuring mountain heights, being very portable and getting rid of the cistern, and the corrections necessary when this is used.

On the Molecular Mobility of Gases. By THOMAS GRAHAM, F.R.S.

The molecular mobility of gases is here considered in reference chiefly to the passage of gases, under pressure, through a thin porous plate or septum, and to the partial separation of mixed gases which can be effected, as will be shown, by such means. The investigation arose out of a renewed and somewhat protracted inquiry regarding the diffusion of gases (depending upon the same molecular mobility), and has afforded certain new results which may prove to be of interest in a theoretical as well as in a practical point of view.

In the diffusiometer, as first constructed, a plain cylinderical glass tube, rather less than an inch in diameter and about ten inches in length, was simply closed at one end by a porous plate of plaster of paris, about one-third of an inch in thickness, and thus converted into a gas receiver.* A superior material for the porous plate is now found in the artificially compressed graphite of Mr. Brockedon, of the quality used for making writing-pencils. This material is sold in London in small cubic masses about 2 inches square. A cube may easily be cut into slices of a millimetre or two in thickness by means of a saw of steel spring. By rubbing the surface of the slice without wetting it upon a flat sand-stone, the thickness may be further reduced to about one-half of a millimetre. A circular disk of this graphite, which is like a wafer in thickness but possesses considerable tenacity, is attached by resinous cement to one end of the glass tube above described, so as to close it and form a diffusiometer. The tube is filled with hydrogen gas over a mercurial trough, the porosity of the graphite plate being counteracted for the time by covering it tightly with a thin sheet of gutta percha. On afterwards removing the latter, gaseous diffusion

* "On the Law of the Diffusion of Gases," Transactions of the Royal Society of Edinburgh, vol. xii. p. 222; or Philosophical Magazine, 1834, vol. ii. pp. 175, 269, 351.

immediately takes place through the pores of the graphite. The whole hydrogen will leave the tube in forty minutes or an hour, and is replaced by a much smaller proportion of atmospheric air (about one-fourth), as is to be expected from the law of the diffusion of gases. During the process, the mercury will rise in the tube, if allowed, forming a column of several inches in height—a fact which illustrates strikingly the intensity of the force with which the interpenetration of different gases is effected. The native or mineral graphite is of a lamelar structure, and appears to have little or no porosity. It cannot be substituted for the artificial graphite as a diffusion-septum. Unglazed earthenware comes next in value to graphite for this purpose.

The pores of artificial graphite appear to be really so minute, that a gas *in mass* cannot penetrate the plate at all. It seems to be molecules only which can pass; and these may be supposed to pass wholly unimpeded by friction, for the smallest pores that can be imagined to exist in the graphite must be tunnels in magnitude to the ultimate atoms of a gaseous body. The sole motive agency appears to be that intestate movement of molecules which is now generally recognised as an essential property of the gaseous condition of matter.

According to the physical hypothesis now generally received,* a gas is represented as consisting of solid and perfectly elastic spherical particles or atoms, which move in all directions, and are animated with different degrees of velocity in different gases. Confined in a vessel, the moving particles are constantly impinging against its sides and occasionally against each other, and such collisions take place without any loss of motion, owing to the perfect elasticity of the particles. Now if the containing vessel be porous, like a diffusiometer, then gas is projected through the open channels, by the atomic motion described, and escapes. Simultaneously the external air or gas, whatever it may be, is carried inwards in the same manner, and takes the place of the gas which leaves the vessel. To the same atomic or molecular movement is due the elastic force, with the power to resist compression, possessed by gases. The molecular movement is accelerated by heat and retarded by cold, the tension of the gas being increased in the first instance and diminished in the second. Even when the same gas is presented both within and without the vessel, and is therefore in contact with both sides of the porous plate, the movement is sustained without abatement—molecules continuing to enter and leave in equal number, although nothing of the kind is indicated by change of volume or otherwise. If the gas in communication be different but possess sensibly the same specific gravity and molecular velocity, as nitrogen and carbonic oxide do, an interchange of molecules also takes place without any change in volume. With gases opposed of unequal density and molecular velocity, the amount of penetration ceases of course to be equal in both directions.

* D. Bernoulli, J. Herapath, Joule, Kronig, Clausius, Clerk Maxwell, and Cazin. The merit of reviving this hypothesis and first applying it to the facts of gaseous diffusion, is fairly due to Mr. Herapath. See "Mathematical Physics," in two volumes, by John Herapath, Esq., (1847.)

These observations are preliminary to the consideration of the passage through a graphite plate, in one direction only, of gas under pressure, or under the influence of its own elastic force. It is to be supposed that a vacuum is maintained on one side of the porous septum, and that air or some other gas, under a constant pressure, is in contact with the other side. Now a gas may pass into a vacuum, in three different modes, or in two modes besides that immediately before us.

1. The gas may enter the vacuum by passing through a minute aperture in a thin plate, such as a puncture in platinum foil made by a fine steel point. The rate of passage of different gases is then regulated by their specific gravities, according to a pneumatic law which was deduced by Professor John Robison from Torricelli's well-known theorem of the velocity of efflux of fluids. A gas rushes into a vacuum with the velocity which a heavy body would acquire by falling from the height of an atmosphere composed of the gas in question, and supposed to be of uniform density throughout. The height of the uniform atmosphere will be inversely as the specific gravity of the gas, the atmosphere of hydrogen, for instance, sixteen times higher than that of oxygen. But as the velocity acquired by a heavy body in falling is not directly as the height, but as the square root of the height, the rate of flow of different gases into a vacuum will be inversely as the square root of their respective densities. The velocity of oxygen being 1, that of hydrogen will be 4, the square root of 16. This law has been experimentally verified.* The times of the effusion of gases, as I have spoken of it, are similar to those of the law of molecular diffusion; but it is important to observe that the phenomena of effusion and diffusion are distinct and essentially different in their nature. The effusion movement affects masses of gas, the diffusion movement affects molecules; and a gas is usually carried by the former kind of impulse with a velocity many thousand times greater than by the latter. The effusion velocity of air is the same as the velocity of sound.

2. If the aperture of efflux be in a plate of increased thickness, and so becomes a tube, the effusion-rates of gases are disturbed. The rates of flow of different gases, however, assume again a constant ratio to each other when the capillary tube is considerably elongated, when the length exceeds the diameter at least 4000 times. These new proportions of efflux are the rates of the "Capillary Transpiration of Gases."† The rates were found to be the same in a capillary tube composed of copper as they are in a tube of glass, and appear to be independent of the material of the capillary. A film of gas no doubt adheres to the inner surface of the tube, and the friction is really that of gas upon gas, and is consequently unaffected by the nature of the tube-substance. The rates of transpiration are not governed by specific gravity, and are indeed singularly unlike the rates of effusion.

The transpiration-velocity of oxygen being 1, that of chlorine

* "On the Motion of Gases," Phil. Trans. 1846 p. 573.

† Phil. Trans. 1846, p. 591, and 1849, p. 349.

1·5, that of hydrogen 2·26, of ether vapor at low temperatures the same or nearly the same number as hydrogen, of nitrogen and carbonic oxide half the velocity of hydrogen, of olefiant gas, ammonia, and cyanogen 2 (double or nearly double that of oxygen), of carbonic acid 1·376, and of the gas of marshes 1·815. In the same gas the transpirability of equal volumes increases with density, whether occasioned by cold or pressure. The transpiration-ratios of gases appear to be in constant relation with no other known property of the same gases, and they form a class of phenomena remarkably isolated from all else at present known of gases.

There is one property of transpiration immediately bearing upon the penetration of the graphite plate by gases. The capillary offers to the passage of gas a resistance analogous to that of friction, proportional to the surface, and consequently increasing as the tube or tubes are multiplied in number and diminished in diameter, with the area of discharge preserved constant. The resistance to the passage of a liquid through a capillary was observed by Poiseuille to be nearly as the fourth power of the diameter of the tube. In gases the resistance also rapidly increases; but in what ratio, has not been observed. The consequence, however, is certain, that as the diameter of the capillaries may be diminished beyond any assignable limit, so the flow may be retarded indefinitely, and caused at last to become too small to be sensible. We may therefore have a mass of capillaries of which the passages form a large aggregate, but which are individually too small to permit a sensible flow of gas under pressure. A porous solid mass may possess the same reduced penetrability as the congeries of capillary tubes. Indeed the state of porosity described appears to be more or less closely approached by all loosely aggregated mineral masses, such as lime-plaster, stucco, chalk, baked clay, non-crystalline earthy powders like hydrate of lime or magnesia compacted by pressure, and in the highest degree perhaps by artificial graphite.

3. A plate of artificial graphite, although it appears to be practically impenetrable to gas by either of the two modes of passage previously described, is readily penetrated by the agency of the molecular or diffusive movement of gases. This appears on comparing the time required for the passage of equal volumes of different gases under a constant pressure. Of the following three gases, oxygen, hydrogen, and carbonic acid, the time required for the passage of an equal volume of each through a capillary glass tube, in similar circumstances as to pressure and temperature, was formerly observed to be as follows:—

	Time of capillary transpiration.
Oxygen, . . .	1
Carbonic acid, . . .	0·72
Hydrogen, . . .	0·44

Through a plate of graphite, of half a millimetre in thickness, the same gases were now observed to pass, under a constant pressure of a column of mercury of 100 millimetres in height, in times which are as follows:—

	Time of molecular passage.	Square root of density (oxygen 1).
Oxygen,	1	1
Hydrogen,	0·2472	0·2502
Carbonic acid,	1·1886	1·1760

It appears then that the times of passage through the graphite plate have no relation to the capillary transpiration-times of the same gases first quoted above. The new times in question, however, show a close relation to the square roots of the densities of the respective gases, as is seen in the last Table; and so far they agree with theoretical *times of diffusion* usually ascribed to the same gases.

The experiments were varied by causing the gases to pass into a Torricellian vacuum, and consequently under the full pressure of the atmosphere. The times of penetration of equal volumes of gases were now:—

	Times.	$\sqrt{\text{Density}}$.
Oxygen,	1	1
Air,	0·9501	0·9507
Carbonic acid,	1·1860	1·1760
Hydrogen,	0·2505	0·2502

This penetration of the graphite plate by gases appears to be entirely due to their own proper molecular motion, quite unaided by transpiration. It seems to offer the simplest possible exhibition of the molecular or diffusive movement. This pure result is to be ascribed to the wonderfully fine porosity of the graphite. The interstitial spaces, or channels, appear to be sufficiently small to extinguish transpiration, or the passage of masses, entirely. The graphite becomes a molecular sieve, allowing molecules only to pass through.

With a plate of stucco, the penetration of gases under pressure is very rapid, and the volumes of air and hydrogen passing in equal times are as 1 to 2·891, which is a number for hydrogen intermediate between its transpiration-volume 2·04 and diffusion-volume 3·8, showing that the passage through stucco is a mixed result.

With a plate of biscuitware, 2·2 millimetres in thickness, the volume of hydrogen rose to 3·754 (air=1), approaching closely to 3·8, the molecular ratio.

The rate of passage of a gas through graphite appeared also to be closely proportional to the pressure.

Further, hydrogen was found to penetrate through a graphite plate into a vacuum, with sensibly the same absolute velocity as it diffused into air, establishing the important fact that the impelling force is the same in both movements. The molecular mobility may therefore be spoken of as the diffusive movement of gases; the passage of gas through a porous plate into a vacuum, as diffusion in one direction or single diffusion; and ordinary diffusion, or the passage of two gases in opposite directions, as double, compound, or reciprocal diffusion.

Atmolysis.—A partial separation of mixed gases and vapors of unequal diffusibility can be effected by allowing the mixture to permeate through a graphite plate into a vacuum, as was to be expected from the preceding views. As this method of analysis has a practical

character and admits of wide application, it may be convenient to distinguish it by a peculiar name. The amount of the separation is in proportion to the pressure, and attains its maximum when the gases pass into a nearly perfect vacuum. A variety of experiments were made on this subject, of which perhaps the most interesting were those upon the concentration of the oxygen in atmospheric air. When a portion of air confined in a jar is allowed to penetrate into a vacuum through graphite or unglazed earthenware, the nitrogen should pass more rapidly than the oxygen in the proportion of 1·0668 to 1 and the proportion of oxygen be proportionally increased in the air left behind in the jar. The increase in the oxygen actually observed when the air in the jar was reduced from 1 volume

To 0·5	volume,	was 0·48	per cent.
0·25	"	" 0·98	"
0·125	"	" 1·54	"
0·0625	"	" 2·02	"

Or, the oxygen increased from 21 to 23·02 per cent. in the last sixteenth part of air left behind in the jar.

The most remarkable effects of separation are produced by means of the *tube atmolyser*. This is simply a narrow tube of unglazed earthenware, such as a tobacco-pipe stem two feet in length, which is placed within a shorter tube of glass and secured in its position by corks, so as to appear like a Liebig's condenser. The glass tube is placed in communication with an air-pump, and the annular space between the two tubes is maintained as nearly vacuous as possible. Air or any other mixed gas is then allowed to flow in a stream along the clay tube, and collected as it issues. The gas so atmolysed is of course reduced in volume, much gas penetrating through the pores of the clay ube into the air-pump vacuum; and the slower the gas is collected he greater the proportional loss. In the gas collected, the denser constituent of the mixture is thus concentrated in an arithmetical ratio, while the volume of the gas is reduced in a geometrical ratio. In one experiment the proportion of oxygen in the air after traversing the atmolyser was increased to 24·5 per cent., or 16·7 upon 100 oxygen originally present in the air. With gases differing so much in density and diffusibility as oxygen and hydrogen, the separation is of course much more considerable. The explosive mixture of two volumes of hydrogen and one volume of oxygen, gave oxygen containing only 9·3 per cent. of hydrogen, in which a taper burned without explosion; and with equal volumes of oxygen and hydrogen, the proportion of the latter was easily reduced from 50 to 5 per cent.

Interdiffusion of Gases—double diffusion.—The diffusiometer was much improved in construction by Prof. Bunsen, from the application of a lever arrangement to raise and depress the tube in the mercurial trough. But the mass of stucco forming the porous plate in his instrument was too voluminous, in my opinion, and, from being dried by heat, had probably detached itself from the walls of the glass tube. The result obtained of 3·4 for hydrogen, air being 1, is, I understand, no longer insisted upon by that illustrious physicist. It is indeed

curious that my old experiments generally rather exceeded than fell short of the theoretical number for hydrogen $\sqrt{\frac{1}{0{\cdot}06926}} = 3{\cdot}7997$.

With stucco as the material, the cavities in the porous plate form about one-fourth of its bulk, and affect sensibly the ratio in question, according as they are or are not included in the capacity of the instrument. Beginning the diffusion always with these cavities filled with hydrogen, the numbers now obtained with a stucco plate of 12 millimetres in thickness, dried without heat, were 3·783, 3·8, and 3·739 when the volume of the cavities of stucco is added to the air and hydrogen, and 3·931, 3·949, and 3·883 when such addition is not made to these volumes. The graphite plate, on the other hand, being thin, and the volume of its pores too minute to require to be taken into account, its action is not attended with the same uncertainty. With a graphite plate of 2 millimetres in thickness, the number for hydrogen into air was 3·876, and of hydrogen into oxygen 4·124, instead of 4. With a graphite plate of 1 millimetre in thickness, hydrogen gave 3·993 to air 1. With a graphite plate of 0·5 millimetre in thickness, the proportional number for hydrogen to air rose to 3·984, 4·067. A similar departure from the theoretical number was observed when hydrogen was diffused into oxygen or carbonic acid, instead of air. All these experiments were made over mercury and with dried gases. It appears that the numbers are most in accordance with theory when the graphite plate is thick, and the diffusion slow in consequence. If the diffusion be very rapid, as it is with the thin plates, something like a current is possibly formed in the channels of the graphite, taking the direction of the hydrogen and carrying back in mass a little air, or the slower gas, whatever it may be. I cannot account otherwise for the slight predominance which the lighter and faster gas appears to acquire in diffusing through the porous septum.

Speculative ideas respecting the constitution of matter.—It is conceivable that the various kinds of matter, now racognised as different elementry substances, may possess one and the same ultimate or atomic molecule existing in different conditions of movement. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies. We know the anxiety with which this point was investigated by Newton, and the care he took to ascertain that every kind of substance, "metals, stones, woods, grain, salts, animal substances, &c.," are similarly accelerated in falling, and are therefore equally heavy.

In the condition of gas, matter is deprived of numerous and varying properties with which it appears invested when in the form of a liquid or solid. The gas exhibits only a few grand and simple features. These again may all be dependent upon atomic and molecular mobility. Let us imagine one kind of substance only to exist, ponderable matter; and further, that matter is devisible into ultimate atoms, uniform in size and weight. We shall have one substance and a common atom. With the atom at rest the uniformity of matter would be

perfect. But the atom possesses always more or less motion, due, it must be assumed, to a primordial impulse. This motion gives rise to volume. The more rapid the movement the greater the space occupied by the atom, somewhat as the orbit of a planet widens with the degree of projectile velocity. Matter is thus made to differ only in being lighter or denser matter. The specific motion of an atom being inalienable, light matter is no longer convertible into heavy matter. In short, matter of different density forms different substances—different inconvertible elements as they have been considered.

What has already been said is not meant to apply to the gaseous volumes which we have occasion to measure and practically deal with, but to a lower order of molecules or atoms. The combining atoms hitherto spoken of are therefore not the molecules of which the movement is sensibly affected by heat, with gaseous expansion as the result. The gaseous molecule must itself be viewed as composed of a group or system of the preceding inferior atoms, following as a unit laws similar to those which regulate its constituent atoms. We have indeed carried one step backward and applied to the lower order of atoms ideas suggested by the gaseous molecule, as views derived from the solar system are extended to the subordinate system of a planet and its satellites. The advance of science may further require an indefinite repetition of such steps of molecular division. The gaseous molecule is then a re-production of the inferior atom on a higher scale. The molecule or system is reached which is affected by heat, the diffusive molecule, of which the movement is the subject of observation and measurement. The diffusive molecules are also to be supposed uniform in weight, but to vary in velocity of movement, in correspondence with their constituent atoms. Accordingly the molecular volumes of different elementary substances have the same relation to each other as the subordinate atomic volumes of the same substances.

But further, these more and less mobile or light and heavy forms of matter have a singular relation connected with equality of volume. Equal volumes of two of them can coalesce together, unite their movement, and form a new atomic groupe, retaining the whole, the half, or some simple proportion of the original movement and consequent volume. This is chemical combination. It is directly an affair of volume, and only indirectly connected with weight. Combining weights are different, because the densities, atomic and molecular, are different. The volume of combination is uniform, but the fluids measured vary in density. This fixed combining measure—the *metron* of simple substances—weighs 1 for hydrogen, 16 for oxygen, and so on with the other "elements."

To the preceding statements respecting atomic and molecular mobility, it remains to be added that the hypothesis admits of another expression. As in the theory of light we have the alternative hypothesis of emission and undulation, so in molecular mobility the motion may be assumed to reside either in separate atoms and molecules, or in a fluid medium caused to undulate. A special rate of vibration or pulsation originally imparted to a portion of the fluid medium enlivens

that portion of matter with an individual existence, and constitutes it a distinct substance or element.

With respect to the different states of gas, liquid, and solid, it may be observed that there is no real incompatibility with each other in these physical conditions. They are often found together in the same substance. The liquid and the solid conditions supervene upon the gaseous condition rather than supersede it. Gay-Lussac made the remarkable observation that the vapors emitted by ice and water, both at 0° C., are of exactly equal tension. The passage from the liquid to the solid state is not made apparent in the volatility of water. The liquid and solid conditions do not appear as the extinction or suppression of the gaseous condition, but something *superadded* to that condition. The three conditions (or constitutions) probably always coexist in every liquid or solid substance, but one predominates over the others. In the general properties of matter we have, indeed, to include still further (1) the remarkable loss of elasticity in vapors under great pressure, which is distinguished by Mr. Faraday as the Caignard-Latour state, after the name of its discoverer, and is now undergoing an investigation by Dr. Andrews, which may be expected to throw much light upon its nature; (2) the colloidal condition or constitution, which intervenes between the liquid and crystalline states, extending into both and affecting probably all kinds of solid and liquid matter in a greater or less degree. The predominance of a certain physical state in a substance appears to be a distinction of a kind with those distinctions recognised in natural history as being produced by unequal development. Liquefaction or solidification may therefore not involve the suppression of either the atomic or the molecular movement, but only the restriction of its range. The hypothesis of atomic movement has been elsewhere assumed, irrespective of the gaseous condition, and is applied by Dr. Williamson to the elucidation of a remarkable class of chemical re-actions which have their seat in a mixed liquid.

Lastly, molecular or diffusive mobility has an obvious bearing upon the communication of heat to gases by contact with liquid or solid surfaces. The impact of the gaseous molecule, upon a surface possessing a different temperature, appears to be the condition for the transference of heat, or the heat movement, from one to the other. The more rapid the molecular movement of the gas the more frequent the contact, with consequent communication of heat. Hence, probably, the great cooling power of hydrogen gas as compared with air or oxygen. The gases named have the same specific heat for equal volumes; but a hot object placed in hydrogen is really *touched* 3·8 times more frequently than it would be if placed in air, and 4 times more frequently than it would be if placed in an atmosphere of oxygen gas. Dalton had already ascribed this peculiarity of hydrogen to the high "mobility" of that gas. The same molecular property of hydrogen recommends the application of that gas in the air engine, where the object is to alternately heat and cool a confined volume of gas with rapidity.

Artificial Sunlight.

From the London Mechanics' Magazine, April, 1864.

We know that for many year past attempts have been made to evolve from natural elements a gas or flame which, on a small scale, should equal in quality and intensity the light emitted from the sun. Abroad, as well as at home, this has been a haunting thought in the brain of chemists. Innumerable experiments, with more or less success for their results, have been made from time to time, and there is no doubt that eventually perseverance will meet its almost unfailing reward—achievement of the object sought.

So long back as 1859, Professor Bunsen and Professor Roscoe suggested that the fusion of the metal magnesium might prove to be the solution of the philosophical problem, and now Mr. Sondstadt is actually commencing to manufacture that metal on a large scale for photographic purposes. It has been demonstrated that, by burning magnesium wire in a spirit or oil lamp, an illuminating power of great brilliancy might be gained. The two professors named, long since examined the photo-chemical action of the sun, compared with a terrestrial source of light, and this latter was that effected by the combustion of magnesium wire. The application of this light may become, it is easy to perceive, of vast importance beyond its photographic uses. A burning magnesium wire, of the thickness of 0·297 millimetre, evolves as much light as seventy-four stearine candles, of which five go to the pound. In order to produce a light equal to that of seventy-four such candles burning for ten hours, and in which 20 lbs. of stearine would be consumed, 72·2 grammes of magnesium wire would be required. The magnesium wire is prepared by forcing out the metal from a heated steel press, having a fine opening at the bottom. For the purpose of consumption, it may be rolled up in coils on a spindle, which, by the agency of clock-work, or weights above, could be made to revolve. A pair of feeding rolls would push the end of the wire forward, at a rate commensurate with the speed of its combustion.

Magnesium is not in itself costly at present, but there is no doubt that the efforts of Mr. Sondstadt and others who are devoting attention to the subject will lessen the expense of its production by improved manipulation.

So far as the usefulness of the discovery is concerned in relation to photography, we have the following testimony from Mr. Brothers, of Manchester:—"The result of an experiment I have just tried, is, that in fifty seconds, with the magnesium light, I have obtained a good negative copy of an engraving—the copy being made in a darkened room. Another copy was made in the usual way, in daylight, and in fifty seconds the result was about equal to the negative taken by the artificial light." Who shall say, therefore, that at some not distant day, nature and science may not place at our disposal a substitute for the bright orb of day—an artificial sun.

Boiler Explosions, and their Prevention.

From the London Mining Journal, No. 1498.

An important letter upon this subject has been addressed to the Editor of the *Times* by Mr. Joseph Hopkinson, of the Britannia Works, Huddersfield, in which he points out the disadvantage of official inspection, whether conducted by the government or by commercial associations. He maintains that many boiler explosions have taken place from the interference of the inspector. He admits that inspection may be the means of lessening these catastrophes, but considers that the class of inspection required must greatly differ from the system carried out by the Manchester Association, which he avers is not only retrograde in its tendency, but is a positive evil. It throws the responsibility into the lap of an irresponsible body, who have merely made boiler explosions a game of speculation. His experience with these societies proves the remarks made by Mr. Jewison, coroner for the Leeds district, that it is "A system by which boiler owners are lulled into fancied security." He continues, that he would ask these societies how many explosions have taken place of boilers under their charge, and how many persons injured, which would have been prevented had there been a proper valve? how many fusible plugs they have found inoperative? and why that apparatus is recommended by one of the Manchester associations, and condemned by the other? and after the report made by the Franklin Institute, declaring its inefficiency, from experiments made. He would ask them why they recommend twice as many fittings to a boiler as is necessary? why they would have so many holes pierced in a boiler for the adoption of the various "philosophical toys" they recommend? He observes that the operative engineers of Oldham and neighborhood adopt a very different course to that pursued by the Manchester Association, which is by the formation of an institution, where lectures and information on matters relating to the steam engine and boiler are imparted to them, and they choose a large body of men of sufficient experience from their own society, who act as inspectors for their neighbors' boilers. The result has been, by this spirit of self-government and the adoption of proper fittings, that not one explosion has taken place—a very different result to that of these great companies, seemingly got up under more favorable auspices. The officialism of red tape and indifference, as well as the payment of useless salaries, is avoided, whilst the results are far more conducive to the interests of society. Some parties, however, adopt a different course to that pursued either by the Manchester Association or the engineers of Oldham, and which they say is preferable to any yet devised. The method is, by agreeing with a practical boiler maker to examine the boiler thoroughly, whenever called upon, at a fixed sum per boiler examination; and should he succeed in discovering any faulty place he is entrusted with the repairs. Mr. Hopkinson's conclusions are, that the only safeguard against boiler explosions is to be found in efficient self-acting mechanical contrivances, and declares that boiler explosions can be prevented from all causes except that of a boiler where it is too weak to withstand its ordinary working pressure; and he is prepared

to attach to any boiler in London, or elsewhere, apparatus such as are in use upon the boilers at the Royal Arsenal, Woolwich, and upon the boilers of Messrs. John Brown & Co., armor-plate manufacturers, Sheffield, 42 in number, and 33 at Messrs. Crossley & Co., Halifax, and the whole of Messrs. Naylor, Vickers & Co.'s boilers, Sheffield; in fact, to 4000 boilers in this and other countries; and he offers £200 to any person who can demonstrate the possibility of exploding a boiler from over-pressure of steam, or deficiency of water, if the boiler be fitted with this patent compound safety-valve, the warnings of which, if disregarded, put a stop to the working of the engine, and render boiler explosions impossible; and what is still of great importance, its action can be stopped only by again supplying the water to the boiler.

A New Process of Engraving.

From the London Mechanics' Magazine, May, 1864.

A M. Dulos has recently invented a new process of engraving, which is described by the *Moniteur Scientifique*, as follows:—A copper plate, on which the design has been traced with lithographic ink, receives, by the action of the pile, a deposit of iron on the parts untouched by the ink; the ink having been removed by means of benzine, the white portions of the design are represented by the layer of iron, and the black by the copper itself; the plate is then plunged into a bath of cyanide of silver, under a galvanic current, and the silver is deposited on the copper only. In this condition mercury is poured over the plate, which attaches itself to the silvered portions only, appearing in relief, and taking the place of the lithographic ink. Then take, in plaster or melted wax, an imprint, the cast of which, presenting the counterpart of the projections of mercury, gives a kind of copperplate engraving. This cast has not sufficient strength to bear the press; but by metallizing the mould, and depositing upon it, electro-chemically, a layer of copper, we obtain an exact re-production of the original projections of mercury, and, in some sort, of a matrix by means of which impressions of the plate may be produced *ad infinitum*.

For typographic engraving (figures in relief), the plate of copper should receive, on leaving the hands of the designer, a layer of silver, deposited only on the parts untouched by the lithographic ink; the ink is removed by benzine, the surfaces first covered by the design are oxidized, and the treatment above described is continued. At the end of the operation the raised portions of the electro-chemical plate intended for the impression will be found to correspond with the tracing of the design, and the hollow portions with the thickenings raised about the design by the mercury.

This process, which is the starting-point and the basis of M. Dulos' invention, has led him to the discovery of some more simple methods, which have led to important practical results, the fusible metal or amalgam of copper substituted for mercury giving rapid and remarkably perfect results.

Artificial Light and Lighting Materials. By B. H. Paul, Esq.

From the Journal of the Society of Arts, No. 593.

(Continued from page 132.)

Illuminating Power.—In the case of all kinds of lighting materials, the light produced when they are burnt, is due to their containing, or yielding when heated, olefiant gas, or other hydrocarbon gases, which are equivalent to it as regards the production of light. In the production of artificial light by any of the means that have been hitherto commonly used, it is always the result of the intense ignition of solid particles of carbon. Luminiferous hydro-carbon gases and vapors, which, for convenience sake, may be collectively represented by the term olefiant gas, all agree in the character of being decomposed at a temperature higher than that requisite for their production, in such a manner that the greater part of the carbon they contain, is set free in a state of very minute division. This is an essential character of these luminiferous substances. In any ordinary luminous flame the particles of carbon so separated become intensely heated, and emit light while being burnt, giving to the flame its whiteness and brilliancy.

The production of heat is, therefore, an essential preliminary to the production of light, and it is also equally necessary for the production of light that the heat produced should act upon some solid substance. The separation of minutely-divided carbon from the luminiferous hydro-carbons, when they are heated, provides this essential condition for the production of light. Gases or vapors which do not yield any solid substance when burnt, do not afford a luminous flame, or at most, only a feeble light. Thus, for instance, hydrogen and marsh gas, or sulphur, burn without evolving any considerable light. Olefiant gas, on the contrary, burns with an intensely white luminous flame. At the same time it is indispensable that the solid particles separated in an ordinary flame should not retain their solid state, but should, by combustion, be converted into gas. The carbon separated in the flame of ordinary lighting materials, burns and is converted into carbonic acid gas. If this were not the case every flame would constantly produce a shower of dust.

The greater the number of solid heated particles in a flame, the greater will be the light it evolves, consequently, the amount of light capable of being produced by various hydro-carbon gases and vapors, or their illuminating power, is directly proportionate to the amount of carbon contained in a given volume of the gas or vapor, and capable of being separated during combustion. Taking olefiant gas as the standard of comparison, and referring to the table of the several hydro-carbon oils constituting petroleum and coal oil, it will be seen that the individual members of the series differ very considerably in this particular, and the figures expressing the relative amount of carbon also express the relative illuminating power of equal volumes of their vapors.

It is probable that in burning the vapor of any one of these substances the general nature of the decomposition which takes place con-

sists in the production of marsh gas which burns, and the separation of all the carbon over and above that requisite for the formation of marsh gas. Consequently, in the flame of the substance, No. 4 of the table, there would be nearly twice as much carbon separated as in the flame of olefiant gas. Separation of carbon to such an extent would be more than sufficient for the production of light under ordinary circumstances, and would be accompanied by a tendency to the production of smoke, in consequence of some of the carbon escaping combustion. This tendency would of course be greater in proportion to the increased amount of carbon in the vapor, and although the illuminating capability would be greater in the same proportion, it would not be possible to realize that capability except under special conditions.

There is, therefore, a practical limit, in regard to the mode in which lighting materials are generally used, beyond which an increase in the amount of carbon in the material would be rather prejudical than otherwise.

In the case of gas, it is never anything like an approximation to pure olefiant gas that is used, but only a mixture of it with other gases, containing from 5 to upwards of 20 per cent. of olefiant gas. The following gives the composition of coal-gas in various localities:—

	Manchester. Roscoe.	Greenock. T. Thomson.			Newcastle. T. Richardson		Boghead. Fyfe.
Olefiant gas, .	8·84	14·50	17·50	20·00	10·19	9·25	27
Marsh gas, .	34·90	66·49	59·94	47·77	31·35	36·05	. .
Hydrogen, .	45·58	12·29	11·46	17·32	28·80	30·17	. .
Carbonic oxide,	6·64	7·07	12·	11·76	16·28	11·42	. .
Carbonic acid, .	3·67	. .	. .	. .	. .	. .	. .
Nitrogen, . .	2·46	. .	. .	. .	. .	. .	. .
Sulph. hydrogen,	0·29	. .	. .	. .	. .	. .	. .

Of these gases, the first three after olefiant gas, are the only ones that take any part in the production of light, the others being mere impurities. Those gases, however, do not contribute directly to the production of light to any extent, but being themselves combustible, serve in part, to produce heat requisite for decomposing the olefiant gas, and rendering the particles of carbon separated from it luminous. They also serve to prevent the flame from smoking, by diluting the olefiant gas, and thus admitting of its being brought more intimately in contact with the air required for perfect combustion. They also give bulk to the flame, and thus diminish the concentration of the light which would take place if the olefiant gas were burnt without any admixture.

In all these respects each one of the three gases answers the purpose required of it equally well or nearly so. There are, however, other particulars in regard to which there is a very considerable difference in their fitness for these purposes. This difference consists in the respective capability of these gases for producing heat and car-

bonic acid by their combustion. Any amount of heat produced by burning any lighting material, over and above that requisite for giving the necessary temperature to the flame, is obviously objectionable, since it would have the effect of heating the atmosphere of the space lighted and rendering it oppressive. Since all lighting materials contain carbon, and by their combustion abstract oxygen from the air, the production of carbonic acid is, to some extent, a necessary result of the use of any such material; but, on account of the prejudicial influence of this gas, it is highly desirable that the quantity produced, for a given quantity of light, should be as small as possible. In both these respects there is a very wide difference between the effects of the three gases which are generally contained in coal gas as the dilutants of the olefiant gas. This will be seen from the following table:—

	Heating power for equal volumes.	Relative volume of Oxygen consumed.	Volume of Carbonic acid produced.	Temperature of Flame.
		Air.		Fahr.
Hydrogen, . .	4,299	0·5 = 2·5	0	4966°
Carbonic acid, . .	3,234	0·5 = 2·5	1	5619
Marsh gas, . .	14,634	2·0 = 10·0	1	4940
Olefiant gas, . .	. .	3·0 = 15·0	2	. .

This comparison serves to show that although hydrogen produces more heat by combustion, and gives a lower temperature in the flame, than carbonic oxide, still it is the best substance to be used for diluting the olefiant gas, since it affords no carbonic acid. On the contrary, marsh gas is worse even than carbonic oxide, on account of its producing much more heat and carbonic acid. In regard to the amount of carbonic acid generated, and the heating effect produced for a given quantity of light, gas, however, has a great advantage over any of the lighting materials formerly used. As compared with tallow, for instance, gas produces only one-half or less than half as much carbonic acid, and little more than one-third the heating effect. But it would be a very great improvement in the manufacture of gas if it could be produced with hydrogen alone as the diluting substance. Many years ago an attempt was made to effect this by Selligue, in Paris, and subsequently the same thing was tried in this country by Professor Donovan and Mr. White, under the name of water gas; but both projects proved unsuccessful, owing to a variety of adverse circumstances.

It will be evident from what has already been said with regard to the dependance of the luminosity of a flame upon its temperature, that it is desirable to produce the highest attainable temperature in the flame, and that any means of increasing that temperature would have the effect of increasing the illuminating effect. A very elegant contrivance for this purpose, in the case of gas, has been devised by Dr. Frankland. It consists in heating the air consumed in the combustion of the gas, by means of the waste heat radiated from the flame, by making it pass down along the side of the chimney before entering at

the bottom of the burner to supply the flame. In this way the air is heated to 500° or 600° F., and there is, of course, a corresponding increase of temperature in the flame. Dr. Frankland has ascertained that by this simple appliance the same amount of light may be obtained with a saving of 49 per cent. of gas, and that for a given consumption of gas there is an increase in the illuminating effect to the extent of 67 per cent.

Returning now to the liquid hydro-carbon oils used for lamps, I will endeavor to point out how the principles I have described in reference to gas obtain in the application of those oils to lighting purposes. In this case the hydro-carbon vapor is not mixed with any diluting substance, as in gas, and all of these oils would give off a considerable amount of smoke if burnt in the same way as fat oils, by reason of the highly carbonaceous nature of their vapors. This circumstance was one of the most serious impediments to the introduction of these oils in the first instance, but eventually a lamp was devised which satisfied all requirements, and which is now largely in use. To understand the way in which the combustion of hydrocarbon oils in these lamps is effected without production of smoke, it must be remembered that in ordinary cases the flame of any lighting material, being for the most part gaseous and in contact with atmospheric air, there is, in accordance with the well-known laws of gaseous diffusion, a continual intermixture of air with the gaseous substances in the flame, and the extent to which this intermixture takes place is determined by several conditions, which may be regulated at pleasure. To take the simplest case, that of gas; the rate of intermixture will be proportionate with the rate at which the gas issues from the burner, and to the velocity of the current of air passing along the sides of the flame. When gas is burnt with a large Argand gas burner it is very liable to smoke, but when a glass cylinder is placed over the flame, the gas burns without smoke, in consequence of the greater intermixture of air with the flame, caused by the draft of the chimney. The degree of draft, and consequently of intermixture of air with the burning gas, will depend upon the height of the chimney. With a very high chimney and powerful draft the intermixture may be effected to such an extent as to neutralize, almost entirely, the illuminating power of the flame. In order, therefore, to effect perfect combustion without loss of lighting effect, it is necessary to proportion these determining conditions of the intermixture of air with the flame, according to the nature of the material to be burnt.

It is in this manner that the combustion of the hydro-carbon oil in lamps is effected. By means of the chimney a strong current of air is produced, and by means of the perforated cone, immediately over the wick, that current of air is made to impinge upon the ascending vapor, and mix it so as to enable it to burn without smoke. In this case, therefore, the air, by means of which combustion is supported, is made to serve the purpose of the diluting gases in ordinary coal gas, giving greater bulk to the gaseous contents of the flame, and effecting such a distribution of the ignited particles of carbon, as to admit of their being perfectly burnt.

The necessity of this admixture of air with the vapor of hydro-carbon oil to enable it to burn without smoke is of course greater in proportion as that vapor is denser and more highly carbonaceous. In this respect it will be seen that the several members of the series of substances constituting these oils differ materially. For instance, the vapor density of No. 16 is twice as great as that of No. 4, and there is the same difference between the amount of carbon in the vapors. The former vapor would also have double the illuminating power of the latter; but the size of the flame of No. 16 would be proportionately smaller, and owing to these two circumstances, the ignited particles of carbon would be so crowded together that a great deal of smoke would be produced unless a copious intermixture of air was effected by means of a very vigorous draft.

Practically the difference between the several substances here referred to, compensate each other in consequence of their being mixed together, in the various kinds of hydro-carbon oils. Thus the higher illuminating power of the denser vapors is to a great extent rendered available by the more bulky nature of the less carbonaceous vapors, and in this way a mean result is arrived at, of both characters combined, which answers all desired purposes. Everything depends upon the due proportionate mixture of the different substances.

From these considerations it will be seen that there is a positive disadvantage in carrying the separation of the more volatile portions of the oil beyond such a point as is indispensably requisite for its being used with safety; for the more those substances are separated from the oil intended to be used in lamps, so much the more must the least volatile portions, at the opposite extremity, be separated from it, in order to obtain a material fit to burn without smoke, and with a sufficiently large flame.

It will now be necessary to consider briefly the applicability for lighting purposes of the hydro-carbon oils which are too volatile to be used in the ordinary lamps. It is a fact that will be familiar to many of the members of this Society, that the idea of applying such highly volatile hydro-carbons as lighting materials is not, by any means, a thing of yesterday. It was first conceived, I believe, long before the year 1830, by a gentleman who has occupied, and still occupies, a most prominent position among those who have contributed to the successful establishment of gas-lighting. Mr. Lowe's original proposal was to use the volatile hydro-carbons obtained from the tar of gas works in the place of water in gas metres, so as to serve the double purpose of measuring the gas supplied to consumers, and of augmenting its illuminating power. Next to him came Professor Donovan, who proposed to make water-gas the medium for vaporizing these liquid hydro-carbons; and he appears to have formed this idea independently of any knowledge of Mr. Lowe's plan of naphthalizing gas. However that may be, the originality of the project in point of time certainly belongs to Mr. Lowe. These projects attracted very great attention at the time; but they never came into anything like general application. It may be that on reflection, the gas companies did not esteem

very highly a plan for doubling the illuminating power of their gas, since that advantage was not necessarily accompanied by a demand for twice as much light as was previously used.

Some years afterwards another attempt was made to employ these highly volatile hydro-carbons as lighting materials, by the late Mr. Mansfield, to whom we are indebted for a very excellent investigation of the chemistry of gas-tar. His plan was to use atmospheric air as the medium for volatilizing the hydro-carbons, chiefly with the view of applying them in this manner for the lighting of country-houses or other places remote from gas-works. The material that he employed for this purpose was the more volatile portion of gas-tar, now known to chemists under the name of benzol; it possessed a character, however, which proved fatal to the undertaking—it was that of becoming solid when considerably cooled. In consequence of this the reduction of temperature produced by its own volatilization rendered it solid, and stopped its further evaporation.

Strangely enough, this very same plan of using atmospheric air saturated with the vapor of a volatile hydro-carbon has quite recently been put forward with much pretence, as a totally new invention, notwithstanding the fact that Mansfield's patent entirely covers this application of these substances, and that as this patent has expired, the process is now public property.

The naphthalizing of coal-gas, though still older, has also been brought before the public lately, under a new name, as being a new invention.

It now only remains to consider how far it may be practicable to use the highly volatile hydro-carbons of petroleum and similar materials for lighting purposes, by diffusing their vapor through atmospheric air. These liquids are free from that prejudicial character of solidifying when cooled, which renders benzol obtained from gas-tar, inapplicable for the same purpose. They are now obtained in tolerable abundance in connexion with the manufacture of hydro-carbon lamp oils, and they are to a great extent still mere waste products, since they do not answer well as a substitute for turpentine, which is the chief use they have yet been put to. It is, therefore, possible that these liquids might be obtained in sufficient quantity and at such cost as would admit of their being used as lighting materials.

By a reference to the table it will be seen that, as compared with olefiant gas, their illuminating power is considerable. Taking the substance No. 7 as representing the available volatile portion of petroleum, its vapor has $3\frac{1}{2}$ times the illuminating power of olefiant gas; in other words, gas containing $3\frac{1}{2}$ per cent. by measure of this vapor would have the same illuminating power as coal-gas containing about 12 per cent. of olefiant gas, and with 7 per cent. of this vapor it would be equal to the best cannel gas.

There is one important point, however, in regard to this application of the volatile hydro-carbons which cannot be overlooked, and which requires to be thoroughly examined before any attempt is made to use them in this way. It is well known that hydro-carbon gases, such as

olefiant gas or marsh gas, the representative of all these hydro-carbons, when mixed with air in certain proportions, form a mixture that is violently explosive. It, therefore, becomes a question of very serious moment whether this fact presents any real or insurmountable obstacle to the use of atmospheric air as the medium for using these volatile oils in the condition of gas—whether, in fact, under the conditions to be observed for such a use of these oils it would be possible to get a mixture of the vapor and of air that would be explosive when brought in contact with a flame. If that be possible, even in some extreme case, that may be of unlikely occurrence, then there can be no doubt that the use of these oils in that way would be highly objectionable.

It cannot be doubted, looking at the mere possibility of the matter, that with these vapors, just as with marsh gas, an explosive mixture might be produced. There is no question as to that. The question really to be considered is, whether such a result could take place under the conditions for using these oils.

It is well-known that the explosibility of a mixture of marsh gas and air depends upon the relative proportion of the gas and air. For its perfect combustion marsh gas requires 10 times its volume of air. Such a mixture is explosive when brought in contact with flame, and the explosive character still continues when the proportion of air is not more than six or seven times as great as the marsh gas. But when marsh gas is mixed with only three or four times its volume of air, the mixture is not at all explosive, but will burn like the unmixed gas. In the same manner, other hydro-carbon gases require to be mixed with a certain proportion of air to form explosive mixtures; and with regard to the vapors of the volatile hydro-carbons, this is equally the case. In consequence of the density of these vapors being so much greater than that of their representative—marsh gas—they require much larger proportions of air for their combustion. Thus for instance, the vapor of the oil No. 7 requires 70 times its volume of air for combustion. In all probability such a mixture would be explosive. But a mixture of this vapor with air which would possess an illuminating power equal to cannel gas, would not contain more than 27 parts of air to one of the vapor. Such a mixture, I believe, would not be explosive under any circumstances that require to be taken into account.

Then as regards the possibility of an explosive mixture being formed, it appears to me highly probable that the extreme volatility of these liquids would in itself constitute a most effectual safeguard against the formation of an explosive mixture. To ensure the efficacy of this character of the oils, however, it would be indispensable that arrangements should be made for securing the intimate contact of the air with the hydro-carbon to be volatilized, and likewise the constant maintenance of a fresh supply of hydro-carbon. All these points, however, are mere matters of detail, which would acquire importance only after the determination of the fact that, with a sufficiently volatile oil, its vapor mixed with air might be burnt like gas without any danger of

an explosive mixture being formed. If that were satisfactorily proved to be the case, it is likely that this mode of using those portions of hydro-carbon oils that are still without any satisfactory application, might be introduced with advantage in many cases. Among others I may mention the lighting of railway carriages, and of houses or public institutions situated at a distance from gas-works.

(To be Continued.)

For the Journal of the Franklin Institute.

On Dr. Brünnow's Magnetic Break Circuit.

By S. W. Robinson, C.E.

AN IMPROVEMENT.

In No. 19 of the "*Astronomical Notices*," edited by the able mathematician and astronomer, Dr. Francis Brünnow, who, till recently, held the position of Professor of Astronomy and Director of the Detroit Observatory, at Ann Arbor, Michigan, is described a new break-circuit invented by himself, and which was adopted at that Observatory, where it has been proved a success. It was also used by him at the Dudley Observatory, while director there.

In entering upon his description he says:

"The contrivances used for breaking the circuit of a chronograph by the pendulum of a clock every second, as far as they are known to me, alter the clock rate considerably wherever they are immediately applied to the pendulum. When this is not the case, as in Mr. Vintle's method, described in No. 1153 of the *Astr. Nachrichten*, or in Mr. R. Bond's new escapement, the clock requires some alterations which it may not always be convenient to introduce. Trying, therefore, to find a contrivance which might be easily applied to the pendulum of any clock without disturbing its uniform rate, I hit upon the idea of using the attractive force of a small magnet connected with the pendulum; and the experiments which I have made with such an apparatus have been so successful that I do not hesitate to recommend the method."

As adopted by him, it consists essentially of a small bar magnet attached to the lower extremity of the pendulum, in a position perpendicular to its plane of vibration, and a small armature arranged in such a manner as to be attracted by one end of the magnet bar as it passes with the vibration of the pendulum. When the armature is attracted, a platinum point, resting against a screw point of platinum, is disconnected, which breaks the circuit of the battery.

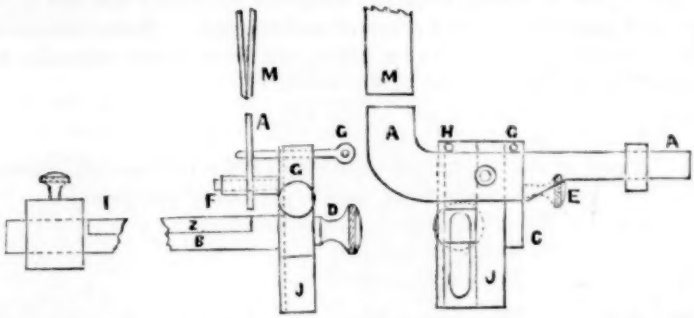
The chief objection to this arrangement of the magnet and armature is the liability of the pendulum to move in a slightly elliptical orbit, the end of the magnet bar passing nearer the armature in one vibration than in the other. This tendency was chiefly obviated by placing a stationary armature opposite the first. But unless in the best adjustment, difficulty was still experienced in the striking of the magnet.

I have improved this arrangement by simply attaching the bar magnet in a vertical position, directly under which is the armature whose

motion breaks the circuit. With this no difficulty can possibly arise from a deviation of the pendulum from its plane of vibration.

It is unknown to me whether the method occurred to Dr. Brünnow earlier than the year 1859, but as far as I have been able to obtain information he was the first who tried it *successfully* and made his results known to the public. At about the same time, Professor C. A. Young, of the Western Reserve College, hit upon the same idea; but after a brief trial with a rude apparatus, pursued the subject no further.

Having been brought in connexion with the use of the astronomical clock and chronograph on the Survey of the North and North-west Lakes, under the superintendence of Colonel Wm. F. Raynolds, A.D.C., and Maj. Engrs., for which clock Mr. Saxton's break-circuit was used,* which seemed to change the rate of the clock considerably for every adjustment of the mercurial connexion, I suggested Dr. Brünnow's method, with the exception of arranging the magnet and armature as described below. After a trial apparatus had been tested, the method was adopted, and a well-finished one for each of the two clocks ordered according to my drawing, an exact copy of which is given here.



M is the bar magnet about two inches in length, composed of two thin pieces of steel plate well magnetized. One is placed† upon each side of the index point of the pendulum, and secured by a small clamp not represented. This attachment is so simple that it can be made to any ordinary clock. A is the armature, or rather a thin steel magnet in itself, presenting opposite polarity to the magnet M. This is carried upon a tube F of brass, or better, platinum, and the whole supported and movable upon a knife-edge of brass or platinum passing through F. There should be a sharp corner in the upper side of the tube for the knife-edge, to prevent the swaying of the armature. H and G are points which limit the motion of the armature. G is supported by an

* With the exception that the tilt hammer plays into a cup of mercury instead of striking a platinum surface. With a battery of three cups, the mercurial contact oxidizes so rapidly as to become necessary to raise the hammer and remove the surface of the mercury several times during a night's observations. In doing so, the hammer is usually raised higher than its motion from the clock. Either from a springing of the wire, or a changed condition of the mercury surface, or both, it is believed that the clock rate has been disturbed.

† North polarity should be placed downward for the better retention of the magnetism.

ivory piece C to insulate it, having a slight vertical motion, and secured by E. With this the motion of A is regulated.

The bearing of the armature against G should be platinum. The brass piece J has a vertical motion for adjusting the distance of the armature from the magnet, and secured by D. B is a bar, simple or compound, connecting the apparatus with the stand I. If the clock has a gridiron pendulum, the simple bar only is necessary; but if the pendulum is compensated with mercury, its lower extremity rises and falls, from the influence of heat, and in order that the distance between the magnet and armature may be constant, I adopted a compound bar, B Z, of brass and zinc, avoiding the use of iron, for the influence it would exert upon the magnet bar M. In order to know the length of the zinc insertion Z, I found the following interesting problem presented to me, viz: *to determine the deflection of a compound bar from the influence of heat.*

I intend to offer more upon this at some future time. My space here is too limited for a full discussion of the subject. I will, however, give the formula with which I calculated the radius of curvature of the bar. This is constant for the whole length of the bar, for a given degree of temperature, and a bar of uniform size. Hence the curve is a circle, and the length can be calculated for a given deflection by a substitution in the equation of the circle.

Let d = the uniform depth of the bar,
b = the uniform breadth,
E and E′ the moduli of elasticity of the two metals, assuming them the same for elongation or compression,
e and e' the co-efficients of expansibility,
ρ = the radius of curvature,
and n = the change of temperature.

Then I get

$$\rho = \frac{d}{2n\,(e-e')}\left(1+\frac{\mathrm{E}+\mathrm{E}'}{2\sqrt{(3\,\mathrm{E}\,\mathrm{E}')}}\right)$$

What can be required of a break-circuit further than that the rate of the clock should be unaffected by it? its easy applicability to any clock; a constancy in its operations; that it should produce short breaks; and its unliability to get out of repair. All these qualities are possessed in a high degree by this break-circuit.

This break-circuit is now in use upon the Survey of the North and North-west Lakes, under the direction of our able and esteemed superintendent, to whom it is yielding the utmost satisfaction, and to whom reference can be had concerning it.

I append here some of Dr. Brünnow's results:

"When a chronograph is used with one writing pen only, it is important to have the breaks very short. With my apparatus I succeeded in making breaks of 0·06 of a second, and as this can never produce a greater error in reading an observation coinciding with a second break, than 0·03 of a second, it is sufficient for all practical purposes.

"My clock before the magnet was attached, and the break-circuit before it, was regulated to sidereal time. With the magnet and break-circuit the observed errors of the clock have been as follows:—

			Daily Rate.
May 3	10h	+26·10	
			—1·28
4	10	24·82	
			—1·25
5	12.2	23·46	
			—1·25
11	13	15·94	
			—1·27
15	14	10·81	
			—1·20
22	12	2·47	
			—1·17
23	12	+1·30	
			—1·26
25	12	—1·22	
			—1·32
29	12	6·52	
			—1·32
June 2	14	—11·90	

"A better rate than this can hardly be expected, and shows that the magnet has no sensible disturbing influence."

Barometers.

From the Journal of the Society of Arts, No. 601.

SIR: In your *Journal* of the 1st of April appears a description of a so-called new barometer, which, I think, will be found accurately described in the following extract from the *Encyclopædia Britannica*, 7th edition, 1842, and written by Sir J. Leslie:—"The most accurate construction of a barometer of this kind [the conical barometer, invented by Amontons, 1695,] is attained by joining together two tubes that have even but unequal bores, the longer and narrower being uppermost. If the width of the upper tube were supposed to be, to that of the under one, as two to three, the scale would be enlarged three times, since, by descending three inches from the top, and, consequently, two at the bottom. the column would suffer a contraction of one inch in height. This species of barometer is thus recommended by its simplicity and its ample range; but the bore of the tube being indispensably small, the mercury moves with difficulty, and resists impressions of minute changes of external action." Also, page 400:—"A modification of the conical barometer in traveling we have ourselves employed. The principal part consists of a small steel stop-cock, a glass tube 31 or 32 inches long, with a bore of the tenth of an inch, sealed at top and filled with quicksilver, is cemented into the one end of the stop-cock, and into the other end is cemented an open and wider tube, 16 inches or more in length, and having its diameter above the eighth of an inch. This compound tube is lodged in a walking-stick, divided into inches and tenths through its whole extent, or only at the upper part." It will thus been seen that this barometer was invented

and published more than 20 years since, and, if necessary, I could mention persons, both in London and elsewhere, who have made them, but probably they have not been much used, from the defect alluded to, which cannot be got rid of, except by using a larger tube, and having a movable piston in the bottom, on Whiting's plan, as exhibited by Mr. Becker last year at the soirée of the Royal Society. Mr. Becker informed me, and, no doubt, others as well, that Whiting had also made them, as described above, with the small tube. Mr. Becker's instrument led, no doubt, to the recent revival of this kind of barometer; but while, perhaps, as a matter of business, it may be thought legitimate for any maker to introduce, in connexion with his own name, an instrument not generally known in the trade, it was hardly to be anticipated that the Royal Society should lend the sanction of their authority to the introduction of an instrument as new which was published more than 20 years since in a by no means obscure work. The above quotations appear also in the eighth edition of the *Encyclopædia*.

I am, &c., W. SYMONS.

Constancy of the Daniel's Battery.

From the London Chemical News, No. 242.

In order to increase still more the constancy of the Daniel's battery, Father Secchi advises the use of fine sand or of powdered sulphur in the porous cell. He accounts for its action by supposing that when the ordinary liquid alone is used there is greater liability to local action taking place upon the zinc. In a battery, the circuit of which is closed for two minutes every quarter of an hour, the learned Father has used an ordinary piece of commercial sheet zinc, half a millimetre in thickness, which has continued in action for more than six months, without showing the least sign of corrosion. For large elements instead of porous diaphragms, he uses bags made of coarse linen cloth, well annointed with a luting of flour and lime. Those who have experienced the difficulty of procuring large porous cells would do well to test these contrivances.

A Case of Dialysation.

To the Editor of the London Chemical News.

Sir:—In preparing solutions of vegetable acids in a large manufactory lately, where, as usual, the solutions are separated from an insoluble residue which is washed until tasteless, I found in one of the filters an impossibility of accomplishing this, the acid taste remaining, although water freely percolated through the mass; and on closer examination I found that the filtered liquor, which originally contained only a very small percentage of sulphuric acid, now consisted almost entirely of it, the crystallizable acid having been kept back by the precipitate, which in this case seems to have acted as a dialyser. I may further add that as soon as there was more space allowed below the false bottom, so that the liquor could not remain at all in contact with the precipitate, this effect ceased.—I am, &c., CHEMICUS.

A Comparison of some of the Meteorological Phenomena of AUGUST, 1864, *with those of* AUGUST, 1863, *and of the same month for* FOURTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	August, 1864.	August, 1863.	August, for 14 years.
Thermometer—Highest—degree, .	96·00°	95·00°	97·00
" " date, .	11th.	10th.	2,'56; 4, '59.
" Warmest day—Mean,	87·50	88·50	88·50
" " " date,	11th.	10th.	10th 1863.
" Lowest—degree, .	61·00	58·00	47·00
" " date, .	31st.	30th.	26th 1856.
" Coldest day—Mean,	68·83	63·00	59·00
" " " date, .	31st.	30th.	26th 1856.
" Mean daily oscillation,	11·71	16·00	15·75
" " " range, .	2·73	3.79	3·72
" Means at 7 A.M , .	75·26	74·95	70·96
" " 2 P.M., .	84·19	85·50	81·39
" " 9 P.M., .	78·43	78·05	74·03
" " for the month,	79·29	79·50	75·46
Barometer—Highest—Inches, .	29·938 in.	30·119 in.	30·255 in.
" " date, . .	19th.	31st.	20th, 1855.
" Greatest mean daily press.	29·915	30·107	30·229
" " date, . .	20th.	31st.	20 & 31 1855.
" Lowest—Inches, .	29·437	29·699	29·356
" " date, . .	3d.	29th.	20th, 1856.
" Least mean daily press.,	29·456	29·762	29·388
" " date, . .	3d.	29th.	20th, 1856.
" Mean daily range, .	0·082	0·100	0·095
" Means at 7 A.M., .	29·726	29·878	29·867
" " 2 P.M., .	29·694	29·850	29·839
" " 9 P.M., .	29·726	29·868	29·858
" " for the month, .	29·715	29·865	29·855
Force of Vapor—Greatest—Inches,	0·895 in.	0·980 in.	1·024 in
" " " date, .	2d.	10th.	1st, 1854.
" " Least—Inches, .	·307	·268	·268
" " " date, .	31st.	30th.	often
" " Means at 7 A.M., .	·633	·585	·587
" " " 2 P.M., .	·630	·590	·594
" " " 9 P.M., .	·673	·610	·615
" " " for the month,	·645	·595	·599
Relative Humidity—Greatest—per ct.,	90·0 per c.	88·0 per c.	100·0 per ct.
" " " date,	21st.	16th.	26th. 1854.
" " Least—per ct.,	38·0	41·0	27·0
" " " date, .	31st.	30th.	1st. 1860
" " Means at 7 A.M.,	71·3	76·8	76·4
" " " 2 P.M.,	53·3	60·0	56·0
" " " 9 P.M.,	68·7	75·0	73.2
" " "for the month	64·4	70·6	68·5
Clouds—Number of clear days,* .	4 days,	10·	9·6
" " cloudy days, .	27 "	21·	21·4
" Means of sky cov'd at 7 A.M.,	70·6 per ct.	55·0 per ct.	55·7 per ct.
" " " " 2 P.M.,	77·4	60·0	60·9
" " " " 9 P.M.,	58·4	45·0	43·1
" " " for the month,	68·8	53·3	53·2
Rain.—Amount,	1·539 in.	1.440 in.	3·889 in.
No. of days on which Rain fell,	9·	9·	9·9
Prevailing Winds—Times in 1000	s 42°39′w·298	N 84°17′w175	s75° 43′w.102

* Sky one-third or less covered at the hours of observation.